SCIENCE

FRIDAY, MAY 19, 1911

CONTENTS

MSS. intended for publication and books, etc., intended for review should be sent to the Editor of SCIENCE, Garrison-on-Hudson, N. Y.

THE BOLYAI PRIZE[1]

THE problems treated by Hilbert are so varied and their importance is so evident that a long preamble seems unnecessary. It is preferable to enter immediately upon the detailed exposition of his principal memoirs. The reader in the presence of results so important will himself draw conclusions.

INVARIANTS

The first works of Hilbert relate to invariants. We know with what passion this part of mathematics was cultivated about the middle of last century and how it has since been neglected. It seemed in fact that Clebsch, Gordan, Cayley and Sylvester had used up all that it was possible to deduce from the old methods and that after them there remained only slight gleanings. But the progress of algebra and arithmetic, and in particular the theory of whole algebraic numbers, the extension soon made of it to integral polynomials, and Kronecker's theory of moduli, made possible the approach of the question from a side still unexplored.

This Hilbert did in attacking at first the celebrated theorem of Gordan, according to which all the invariants of a system of forms can be expressed in a rational and integral way as functions of a finite number of them.

We could not better measure the advance made than by comparing the volume Gordan had to devote to his demonstration with the few lines with which Hilbert has been satisfied. The method gained in gen-

[1] Report on the works of Hilbert by Poincaré. Translated by G. B. Halsted.

erality as much as in simplicity and one could make out a whole series of possible generalizations. A very simple lemma inspired by Kronecker's ideas had made this result possible.

Consider an indefinite series of forms F depending upon n variables; we can find among these a finite number of forms $F_1, \cdots, F_p$, such that any form F of the series can be equated to

$$F \equiv A_1F_1 + \cdots A_pF_p, \tag{1}$$

the A's being forms depending upon the same variables. This is a consequence of the fundamental notion of the modulus introduced by Kronecker. This means, in Kronecker's language, that the divisors common to many moduli, even were they infinite in number, are submultiples of one of them which is their greatest common divisor, and in geometric language (supposing four variables and regarding them as homogeneous coordinates of a point in space) that the aggregate of points common to an infinite number of algebraic surfaces is composed of a finite number of isolated points and a finite number of skew algebraic curves.

But this is not all; suppose the F's are the invariants of a system of forms and the A's functions of the coefficients of these forms.

We may always suppose that the A's are also invariants, otherwise we could perform an arbitrary linear transformation upon the forms. Then in the relation (1) thus transformed would appear the coefficients of this transformation. In applying to the relation (1) transformed a certain train of successive differentiations (the differentiations are performed with respect to the coefficients of the linear transformation) we reach a relation of the same form as (1) but where the A's are invariants. From this the proof of Gordan's theorem follows immediately.

But this is not all; among these fundamental invariants there is a certain number of relations called syzygies. All the syzygies can be deduced from a finite number of them by addition and multiplication. Among these fundamental syzygies of the first order there are syzygies of the second order, which can also be obtained from a finite number of them by addition and multiplication, and so on.

Hilbert gets this result from a general theorem of algebra. Consider a system of linear equations of the form

$$\Sigma F_{ik}X_i = 0,$$

where the F's are given forms and the X's unknown forms homogeneous in regard to certain variables; the study of the solutions of this system and of the relations which connect them leads to the consideration of a series of derived systems continued until we reach a derived system which no longer admits of any solution. Thus it was that Hilbert was led to determine and to study the number $X(R)$ of distinct conditions which a form of degree R should satisfy to be congruent to zero with regard to a given modulus.

But to complete the theory it was not enough to establish the existence of a system of fundamental invariants; it was necessary to give the means of actually forming this, and this problem was made by our author to depend upon a question which connects it with the theory of whole algebraic numbers extended to integral polynomials.

The problem is thus broken up into three others.

1. To find invariants J_m as functions of which all the others can be expressed in *algebraic and integral* form, that is to say, such that any invariant J satisfies an algebraic equation

$$J^k + G_1J^{k-1} + G_2J^{k-2} + \ldots + G_{k-1}J + G_k = 0,$$

the *G's* being polynomials integral with regard to J_m.

2. To find invariants as functions of which all the others can be expressed rationally.

3. To find invariants as functions of which all the others can be expressed in rational and integral form.

Of these three problems the first is the most difficult. If it be supposed solved, the aggregate of invariants presents itself as an *algebraic corpus,* and the first step to make is to determine the degree of this corpus; this it is at which Hilbert arrives at least for binary forms by evaluating in two different ways the number $\phi(\sigma)$ of invariants linearly independent of degree σ, or rather the asymptotic value of this numeric function $\phi(\sigma)$ for σ very great.

The first problem once solved, the solution of the other two goes back to a classic question of the arithmetic of polynomials and of the theory of algebraic corpora. The question is then to find the fundamental invariants by whose aid all the others can be expressed in algebraic and integral form.

With this purpose Hilbert remarks that these are those which can not be annulled without annulling all the others. So we see that the search for these fundamental invariants will be singularly facilitated by the study of *null forms,* that is to say, of those whose numeric coefficients are chosen in such a way that the numeric values of all the invariants may be null.

In the case of binary forms, the null forms are those which are divisible by a sufficiently high power of a linear factor; but in the other cases the problem is more delicate. Our author first establishes a certain number of theorems.

Consider a form with numeric coefficients and its transform by any linear substitution; the coefficients of this transform will be integral polynomials with regard to the coefficients of the substitution. If the determinant of the substitution is an *algebraic and integral* function of these integral polynomials, the proposed form is not a null form. In the contrary case, it is a null form.

Consider, on the other hand, the transforms of a form by a linear substitution depending upon an arbitrary parameter t and in such a way that the coefficients of this substitution are series developable in positive or negative integral powers but increasing with this parameter. If it be a question of a null form, we can choose a substitution of this kind of such a sort that its determinant becomes infinite for $t=0$, while the coefficients of the form transformed remain finite. Hilbert shows that this condition is necessary in order that the proposed form may be null, and it is evident, moreover, that it is sufficient. To each null form corresponds therefore one and perhaps several linear substitutions having the enunciated property. This settled, our author proves that, starting from any null form, we can by a linear transformation, transform it into a *canonic* null form. A form is called canonic when the linear substitution which corresponds to it and which possesses in relation to it the property we have just stated is of the simple form

$$\begin{vmatrix} t^{\lambda_1} & 0 & 0 \\ 0 & t^{\lambda_2} & 0 \\ 0 & 0 & t^{\lambda_3} \end{vmatrix}.$$

The investigation of null forms is thus made to depend on that of canonic null forms which is much more simple. We find that the canonic null forms are those in which certain terms are lacking; and the determination of the terms which should be lacking can easily be made, thanks to a simple geometric scheme.

We see under what a new and elegant aspect present themselves to-day, thanks to Hilbert, problems so many geometers had for fifty years attempted.

THE NUMBER e

Hermite was the first to prove that the number e is transcendant, and shortly afterward Lindemann extended this result to the number π.

This was a victory important for science, but Hermite's methods were still susceptible of betterment; however ingenious and however original they were, one felt they did not lead to the goal by the shortest way. This shortest way Hilbert has found, and it seems that henceforth no one can hope to give new simplification to the proof.

This was the second time that Hilbert had given, of a theorem known but only established by means most arduous, a proof of astonishing simplicity. This faculty of simplifying what had seemed at first complex thus presents itself as one of the characteristics of his genius.

ARITHMETIC

The arithmetical works of Hilbert pertain principally to algebraic corpora. The aggregate of numbers which can be expressed rationally as functions of one or several algebraic numbers constitutes a domain of rationality, and the aggregate of the numbers of this domain which are algebraic integers constitutes a corpus. If we consider then all the algebraic numbers of a corpus which can be put under the form

$$a_1x_1 + a_2x_2 + \cdots + a_px_p$$

where the a's are *given* numbers of the corpus, and the x's indeterminate numbers of the same corpus, the aggregate of these numbers is what is called an *ideal*. That which gives interest to this consideration is that the ideals obey in what concerns their divisibility the ordinary laws of arithmetic and that in particular every ideal is decomposable in one way and only one into ideal primes. This is the *fundamental theorem of Dedekind.*

On the other hand, we may consider numbers which satisfy an algebraic equation of which the coefficients belong to a domain D of rationality. These numbers and those rationally expressible by means of them define a new domain of rationality D' more extended than D; and an algebraic corpus K' more extended than the corpus K which corresponds to D. We then may relate the corpus K', not to the ordinary rational numbers and to the corpus of the integers of ordinary arithmetic, but to the domain D and to the algebraic corpus K. We then may speak of the *relative* degree of K' with reference to K, of the *relative norm* of an algebraic number of K' with reference to K, etc. There will be corpora relatively quadratic obtained by the adjunction to the domain D' of a radical $\sqrt{\mu}$, μ being a number of the domain D, and corpora relatively abelian, obtained by the adjunction to D of the roots of an abelian equation. This is a sort of generalization of the ideas of Dedekind, that Hilbert is doubtless not the first to have seen, but from which he has drawn an unexpected advantage.

We should also speak of galois corpora, whose generating equation is a galois equation. Any corpus is contained in a galois corpus, in the same way the corpus K of which we have just spoken is contained in the corpus K'; and this galois corpus is easily obtained by adjoining to the domain of rationality, not only one of the roots of the generating algebraic equation of K, but *all* its roots.

Questions relative to any corpus are

thus made to depend upon the analogous problems for the galois corpora.

After having shown how we may, by the discussion of a congruence, form all the ideals of given norm, Hilbert has sought a new proof of the fundamental theorem of Dedekind; he established it first for the galois corpora and then easily extended it to any corpus.

Thus Hilbert was led to study the general theory of galois corpora, and he introduced a host of new notions, defining a series of subcorpora, corresponding to different subgroups of the galois group of the generating equation; these subgroups are defined by certain relations they have with any ideal prime of the corpus, and the study of these subgroups opens for us glimpses new and interesting of the structure of the corpus.

Our author gave in 1896 a new proof of Kronecker's theorem according to which the roots of abelian equations can be expressed by the roots of unity. This demonstration purely arithmetical puts in evidence the way of constructing all the abelian corpora of a given group and discriminant.

But the works of Hilbert have had as their principal object the study of corpora relatively quadratic and relatively abelian.

One of the essential points of the theory of numbers is Gauss's law of reciprocity in the subject of quadratic residues; we know with what predilection the great geometer returned to this question and how he multiplied demonstrations.

This law of reciprocity is capable of interesting generalizations when we pass from the domain of ordinary rational numbers to a domain of any rationality. Hilbert has succeeded in realizing this generalization in the case where the corpus is imaginary and has an odd number of classes. He has introduced a symbol analogous to that of Legendre, and the law of reciprocity reached by him presents itself in a simple form; the product of a certain number of such symbols must equal 1.

This generalization presents all the more interest since our author has succeeded in showing that there are genera corresponding to half of all the imaginable systems of characters, a result which should be likened to that of Gauss and which makes possible the extension to a domain of any rationality of this notion of the genus of quadratic forms which is the subject of one of the most attractive chapters of the "Disquisitiones Arithmeticæ."

To go farther, Hilbert is obliged to introduce a new notion and modify the definition of class.

Two ideals belong to the same class in the old or broad sense if their ratio is any existing algebraic number; they belong to the same class in the new or narrow sense if their ratio is an existing algebraic number *which is positive as well as all its conjugates.* The numbers of classes, whether understood in the broad sense or in the narrow sense, are evidently in intimate relation and our author explains what the nature of this relation is. But this new definition allows Hilbert to express in simpler language the theorems he had in view. These theorems stated in their most general form are, as Hilbert says, remarkably simple and of crystalline beauty; their complete proof appeared to our author as the final aim of his studies on algebraic corpora. It is in this general form we shall state them.

If k is any corpus, there is a group Kk which may be called its *class corpus.* Its relative degree is equal to the number of classes in the narrow sense. It is non-ramified, that is to say, no ideal prime of k is divisible by the square of an ideal prime of Kk, and it contains all the non-ramified

corpora relatively abelian with regard to k.

Its relative group is isomorphic to the abelian group which defines the composition of the classes of ideals of k.

The ideal primes of k, although prime in relation to k, are not in general prime in relation to Kk; they may, therefore, be broken into factors ideal primes with regard to Kk, the number of these factors and the power to which they are raised, in a word the mode of partition, depending solely upon the class to which the ideal considered belongs in the corpus k.

Call "*ambige*" a number of Kk which is positive as well as all its conjugates, and which differs from these conjugates only by a factor which is a complex unity.

Each ambige of Kk corresponds to an ideal of k and reciprocally. This property is characteristic of the corpus Kk among all the corpora relatively abelian with regard to k.

We see the bearing of these theorems and the light thrown on the notion of class, since the mutual relations of classes of ideals are reproduced as in a faithful picture by those of the algebraic integers of a corpus.

In reality Hilbert has completely proved these theorems only in particular cases, but these particular cases are very numerous, exceedingly varied and broadly extended. He is, besides, he says, convinced that his methods are applicable to the general case. While sharing his conviction, we must make reservation, so long as this hope, legitimate as it may be, has not been actually realized.

We have spoken above of the law of reciprocity relative to quadratic residues; we must add that Hilbert has given an analogous law for his residues of any power, at least for certain particular corpora.

Summarizing, the introduction of ideals by Kummer and Dedekind was an important advance; it generalized and at the same time cleared up the classic results of Gauss on quadratic forms and their composition. The works of Hilbert we have just analyzed constitute a new step in advance, not less important than the first.

THEOREM OF WARING

Let us speak now of another entirely different arithmetical work. It pertains to proving Waring's theorem according to which every integer can be broken into a sum of N nth powers, N depending only upon n, just as, for example, it can always be broken into a sum of four squares. Needless to recall that this theorem up to the present had simply been stated.

What above all deserves to fix the attention in Hilbert's proof is that it rests on a new way of introducing continuous variables into the theory of numbers.

We start from an identity where a 25uple integral is equated to the mth power of the sum of five squares. Breaking up the domain of integration into smaller domains so as to have a series of approximate values of the integral, as if it were a question of evaluating it by mechanical quadratures, and by the methods of passing to the limit familiar to our author, we reach another identity

$$(x_1^2 + \cdots + x_5^2)^m = \Sigma r_h Y_h^{2m},$$

where the r_h's are rational positive numbers and the Y's linear functions of the x's with integral coefficients. The coefficients r and those of the Y's, as also the number of these linear functions, depend only upon m.

Up to this point we have not gone out of algebra, if not in showing that the coefficients r and those of Y are rational. To get further our author establishes a series of lemmas whose statement is too complicated to be here reproduced and which

lead finally to the complete proof of the theorem. We can not doubt that these considerations, which allow also the obtaining of arithmetical relations in making them come from identities where definite integrals figure, can some day, when we shall have grasped their meaning, be applied to problems much more extended than that of Waring.

GEOMETRY

I come to Hilbert's works so very original on the foundations of geometry.

There are in the history of this geometric philosophy three principal epochs; the first is that where thinkers at whose head we should cite Bolyai founded the non-euclidean geometry; the second is that in which Helmholtz and Lie showed the rôle in geometry of the notion of motion and of group; the third was inaugurated by Hilbert.

The German author takes the logical point of view. What are the axioms enunciated and those unconsciously assumed; what is their real logical content and what may be deduced from them by the simple application of the rules of logic and without new appeal to intuition? Finally, are they independent, or can we on the contrary, deduce them from one another? These are the questions to face.

Hilbert commences, therefore, by establishing the complete list of assumptions, striving not to forget a single one. That is not as easy as one might think, and Euclid himself uses some he does not state. Geometric intuition is so familiar to us that we use intuitive verities, so to speak, without our perceiving them; hence to attain the aim Hilbert proposed to himself, the necessity of not according to intuition the least place.

The savant professor divides the assumptions into five groups:

I. Assumptions of Association (I shall translate by *projective assumptions* in place of seeking a literal translation, as, for example, *assumptions of connection*, which would not be satisfactory).

II. Betweenness assumptions (assumptions of order).

III. Congruence assumptions or metric assumptions.

IV. Euclid's postulate.

V. The Archimedes assumption.

Among the projective assumptions we distinguished those of the plane and those of space; the first come from the well-known proposition: *through two points passes one straight, and only one.*

Going on to the second group, the order assumptions, here is the statement of the first two:

"If three points are on the same straight, they have a certain relation which we express by saying that one of the points, and only one, is between the other two. If C is between A and B, if D is between A and C, D will also be between A and B, etc."

Here still we note that intuition is not brought in; we seek not to fathom the meaning of the word *between*, every relation satisfying the assumptions may be designated by this same word.

The third group comprises the metric assumptions where we distinguish three subgroups, relative respectively to sects, to angles and to triangles.

An important point here was not stressed (in the first German edition, though it appears in the French translation). To complete the list of assumptions it needs to be said that the sect AB is congruent to the inverse sect BA. This assumption implies the symmetry of space and the equality of the angles at the base in an isosceles triangle. Hilbert does not here treat this question, but he has

made it the subject of a memoir to which we shall return later.

The fourth group contains only Euclid's postulate.

The fifth group comprises two assumptions; the first and most important is that of Archimedes.

Let there be any two points A and B on a straight d; let a be any sect; construction d, starting from the point A, and in the sense AB, a series of sects all equal to one another and equal to a:

AA_1, A_1A_2, $\cdots$ $A_{n-1}A_n$; we can always take n so great that the point B is on one of these sects.

This is to say that, if we take any two sects l and L, we can always find a whole number n so great that by adding the sect l to itself n times, we obtain a total sect greater than L.

The second is the assumption of completeness of which I shall explain the meaning further on.

INDEPENDENCE OF THE ASSUMPTIONS

The list of assumptions once drawn up, it is necessary to see if it is free from contradictions. We well know that it is, since geometry exists; and Hilbert first replied yes by constructing a geometry. But strange to say, this geometry is not exactly ours, his space is not ours, or at least is only a part of it. In Hilbert's space are not all the points which are in ours, but only those that, starting from two given points, we can construct with ruler and compasses. In this space, for example, there is no angle of 10°.

In his second edition Hilbert tried to fill out his list so as to obtain our geometry and no other, and so he introduced the assumption of completeness which he states as follows:

To the system of points, straights and planes it is impossible to adjoin another system of objects such that the complete system satisfies all the other assumptions.

It is evident then that this space of which I spoke, which does not contain all the points of our space, does not satisfy this new axiom, because we can adjoin to it those points of our space which it does not contain, without its ceasing to satisfy all the assumptions.

There is, therefore, an infinity of geometries which satisfy all the assumptions except the assumption of completeness, but only one, ours, which satisfies also this latter assumption.

We then must ask if the assumptions are independent, that is to say, if we could sacrifice one of the five groups, retaining the other four, and nevertheless obtain a coherent geometry. Thus it is, suppressing group IV. (Euclid's postulate), we obtain Bolyai's non-euclidean geometry.

We can equally suppress group III. Hilbert has succeeded in retaining groups I., II., IV. and V., as also the two subgroups of the metric assumptions of sects and angles, while rejecting the metric assumption of triangles, that is to say, the proposition III., 6.

Non-archimedean Geometry.—But Hilbert's most original conception is that of non-archimedean geometry, where all the assumptions remain true save that of Archimedes. For this it is needful first to make a *system of non-archimedean numbers*, that is to say, a system of elements between which we can conceive relations of equality and inequality, and to which we can apply operations corresponding to arithmetical addition and multiplication, and this in a way to satisfy the following conditions:

1. The arithmetical rules of addition and of multiplication (commutativity, associativity, distributivity, etc., *arithmetical*

assumptions of combination) remain without change.

2. The rules of the calculus and transformation of inequalities (arithmetical assumptions of ordering) likewise remain.

3. The Archimedes assumption is not true.

We may attain this result by choosing for elements series of the following form:

$$A_0t^m + A_1t^{m-1} + A_2t^{m-2} + \cdots,$$

where m is an integer positive or negative and where the coefficients A are real, and convening to apply to these series the ordinary rules of addition and of multiplication. It is necessary then to define the conditions of inequality of these series so as to *arrange* our elements in a determined order. We shall attain this by the following convention: we will attribute to our series the sign of A_0 and we will say that a series is smaller than another when, if taken away from this, it gives a positive difference.

It is clear that with this convention the rules of the calculus of inequalities hold good, but the Archimedes assumption is no longer true.

Our common numbers come in as particular cases among these *non-archimedean numbers*. The new numbers intercalate themselves, so to speak, in the series of our common numbers, in such a way that there is for example an infinity of new numbers less than a given common number A and greater than all the common numbers less than A.

That settled, imagine a tri-dimensional space wherein the coordinates of a point are measured not by common numbers, but by non-archimedean numbers, but where the usual equations of the straight and of the plane hold good, as also the analytic expressions for angles and sects. It is clear that in this space all the assumptions remain true, save that of Archimedes.

On any straight between our common points would intercalate themselves new points. Likewise there will be on this straight an infinity of new points to the right of all the common points. In a word, our common space is only a part of non-archimedean space.

We see what is the bearing of this invention and wherein it constitutes in the progress of our ideas a step almost as bold as that which Bolyai made us take; the geometry non-euclidean respected, so to speak, our qualitative conception of the geometric continuum while it overturned our ideas on the measure of this continuum. The non-archimedean geometry destroys this conception; it dissects the continuum to introduce into it new elements.

In this conception so audacious Hilbert had had a precursor. In his foundations of geometry Veronese had had an analogous idea. Chapter VI. of his introduction is the development of a veritable non-archimedean arithmetic and geometry where the transfinite numbers of Cantor play a preponderant rôle. Nevertheless, by the elegance and simplicity of his exposition, by the depth of his philosophic views, by the advantage he has derived from the fundamental idea, Hilbert has made the new geometry his own.

Non-arguesian Geometry.—The fundamental theorem of projective geometry is the theorem of Desargues.

Two triangles are called *homologs* when, the straights joining each to each, the corresponding vertices are copunctal. Desargues proved that the intersection points of the corresponding sides of two homologous triangles are costraight; the dual is equally true.

The theorem of Desargues can be established in two ways:

1. By using the projective assumptions of the plane and the metric assumptions of the plane.

2. By using the projective assumptions of the plane and those of space.

Therefore, the theorem could be discovered by a two-dimensional animal, to whom a third dimension would seem as inconceivable as to us a fourth; who consequently would not know the projective assumptions of space, but who would have seen displaced in the plane he inhabited rigid figures analogous to our solid bodies, and who consequently would know the metric assumptions. Equally well the theorem could be discovered by a tri-dimensional animal who should know the projective assumptions of space, but who, having never seen solid bodies displaced, would not know the metric assumptions.

But would it be possible to establish the theorem of Desargues without using either the projective assumptions of space or the metric assumptions, but only the projective assumptions of the plane? It was thought not, but we were not sure. Hilbert has settled the question by constructing a *non-arguesian geometry,* which is of course a plane geometry.

Non-pascalean Geometry.—Hilbert does not stop there; he introduces still a new conception. To understand it, we must return a moment to the domain of arithmetic. We have above seen the notion of number enlarged by the introduction of *non-archimedean numbers.* We need a classification of these new numbers, and to get it we first classify the assumptions of arithmetic into four groups:

1. The laws of associativity and of commutativity of addition, the associative law for multiplication, the two laws of distributivity of multiplication; or, to summarize, all the rules of addition and of multiplication, save the law of the commutativity of multiplication.

2. The assumptions of order, that is to say, the rules of the calculus of inequalities.

3. The law of commutativity of multiplication according to which we can invert the order of the factors without changing the product.

4. The Archimedes assumption.

The numbers which admit the first two groups are called *arguesian;* they may be *pascalean* or *non-pascalean,* according as they satisfy or do not satisfy the assumption of the third group; they will be *archimedean* or *non-archimedean,* according as they satisfy or do not the assumption of the fourth group. We soon shall see the reason for these names.

The ordinary numbers are at once arguesian, pascalean and archimedean. We can prove the law of commutativity from the assumptions of the first two groups and the Archimedes assumption; so there are no numbers arguesian, archimedean and not pascalean.

On the other hand, it is easy to make a system of numbers arguesian, non-pascalean and non-archimedean. The elements of this system will be series of the form

$$S = T_0 s^n + T_1 s^{n-1} + \cdots,$$

where s is a symbol analogous to t, n an integer positive or negative, and T_0, T_1, $\cdots$ numbers of the system T. If therefore we replace the coefficients T_0, T_1, $\cdots$ by the corresponding series in t, we shall have a series depending at the same time upon t and upon s. We add these series S according to the ordinary rules, and likewise for the multiplication of these series, we shall admit the rules of distributivity and of associativity, but we shall hold that the law of commutativity is not true and that, on the contrary, $st = -ts$.

It remains to *arrange* these series in an order so determined as to satisfy the assumptions of order. For that, we give to the series S the sign of the first coefficient

T_0; we shall say that a series is less than another, when if taken away from this, it gives a positive difference. This, therefore, is always the same rule: t is looked upon as very great with regard to any ordinary real number, and s is looked upon as very great with regard to any number of the system T.

The law of commutativity not being true, these now are non-pascalean numbers.

Before going farther I recall that Hamilton long ago introduced a system of complex numbers where the multiplication is not commutative; these are the *quaternions,* which the English so often use in mathematical physics. But, for quaternions the assumptions of order are not true; what therefore is original in Hilbert's conception is that his new numbers satisfy the assumptions of order without satisfying the rule of commutativity.

To return to geometry. Admit the assumptions of [the first] three groups, that is to say, the projective assumptions of the plane and of space, the assumptions of order, and Euclid's postulate; the theorem of Desargues will follow from them since it is a consequence of the projective assumptions of space.

We wish to establish our geometry *without using metric assumptions;* the word *length* therefore has now for us no meaning; we have no right to use the compasses; on the other hand, we may use the ruler, since we admit that we may pass a straight through two points, in virtue of one of the projective assumptions; equally we know how through a point to draw a parallel to a given straight, since we admit Euclid's postulate. Let us see what we can do with these resources.

We can define the homothety (perspective similarity) of two figures; and through it proportion. We can also define equality in a certain measure.

The two opposite sides of a parallelogram shall be equal *by definition;* thus we know how to recognize whether two sects are equal to one another, *provided they be parallel.*

Thanks to these conventions, we now are prepared to compare the lengths of two sects, but *with the proviso that these sects be parallel.*

The comparison as to length of two sects differing in direction has no meaning, and there would be needed, so to speak, a different unit of length for each direction. It is unnecessary to add that the word *angle* has no meaning.

Sects will thus be expressed by numbers; but necessarily these will not be ordinary numbers. All we can say is that if the theorem of Desargues is true, as we suppose, these numbers will belong to an *arguesian system.*

Inversely, having given any system S of arguesian numbers, we can make a geometry such that the lengths of the sects of a straight may be exactly expressed by these numbers.

The equation of the plane will be a linear equation as in the ordinary analytic geometry; but since in the system S multiplication will not be commutative, in general it is needful to make a distinction and to say that in each of the terms of this linear equation the coordinate will play the rôle of multiplicand, and the constant coefficient the rôle of multiplier.

Thus to each system of arguesian numbers will correspond a new geometry satisfying the projective assumptions and those of order, the theorem of Desargues and Euclid's postulate. What now is the geometric meaning of the arithmetical assumption of the third group, that is to say, of the rule of commutativity of multiplication?

The geometric translation of this rule is

Pascal's theorem; I mean the theorem about the hexagon inscribed in a conic, supposing that this conic reduces to two straights. So Pascal's theorem will be true or false according as the system S is pascalean or non-pascalean; and since there are non-pascalean systems, *there likewise are non-pascalean geometries.* The theorem of Pascal can be deduced from the metric axioms; it therefore will be true if we suppose figures may be transformed not only by homothety and translation, as we have done, but also by rotation. Pascal's theorem can likewise be deduced from the Archimedes axiom, since we have seen that every system of numbers arguesian and archimedean is at the same time pascalean; *every non-pascalean geometry is therefore at the same time non-archimedean.*

The Sect-carrier.—We cite still another conception of Hilbert's. He studies the constructions we can make, not with ruler and compasses, but with ruler and a special instrument which he calls the *sect-carrier,* and which enables us to set off on a straight a sect equal to another sect taken on another straight. The *sect-carrier* is not the equivalent of the compasses; this latter instrument enables us to construct the intersection of two circles, or of a circle and any straight; the *sect-carrier* will only give us the intersection of a circle and a straight *passing through the center of this circle.* Hilbert seeks therefore what are the constructions which are possible with these two instruments, and he reaches a very remarkable conclusion.

The constructions which can be achieved with ruler and compasses can likewise be made with the ruler and the *sect-carrier, provided these constructions are such that their result is always real.*

It is evident in fact that this condition is necessary, because a circle is always cut *in two real points* by a straight drawn through its center. But it was hard to foresee that this condition would likewise be sufficient.

But this is not all; in all these constructions, as Kürschák first noticed, it is possible to replace the *sect-carrier* by the *unit-sect carrier,* an instrument which enables us to set off on any straight from any point of it, no longer any sect, but a sect equal to unity.

An analogous question is treated in another article of Hilbert's: *On the equality of the angles at the base of an isosceles triangle.*

In the ordinary plane geometry, the plane is symmetric, which expresses itself in the equality of the angles at the base of the isosceles triangle.

We should make this *symmetry of the plane* appear in the list of metric assumptions. In all the geometries more or less strange of which we have spoken hitherto, in those at least where we admit the metric assumptions, in the non-archimedean metric geometry, in the new geometries of Dehn, in those which are the subject of the memoir "On a New Foundation, etc.," this symmetry of the plane is always supposed. Is it a consequence of the other metric assumptions? Yes, as Hilbert shows, if we admit the Archimedes assumption. No, in the contrary case. There are non-archimedean geometries where all the metric assumptions are true with the exception of this of the symmetry of the plane.

In this geometry it is not true that the angles at the base of an isosceles triangle are equal; it is not true that in a triangle one side is less than the sum of the other two; the theorem of Pythagoras about the square on the hypothenuse is not true. That is why this geometry is called *non-pythagorean.*

I come to an important memoir of Hil-

bert's which is entitled "Foundations of Geometry," which bears then the same title as his "Festschrift," but where he takes, however, a wholly different point of view. In his "Festschrift," in fact, as we have seen by the preceding analysis, the relations of the notion of space and the notion of group resulting from the works of Lie are laid aside or relegated to an inferior place. The general properties of groups do not appear in the list of fundamental assumptions. Not so in the memoir of which we are to speak.

As regards the ideas of Lie, the progress made is considerable. Lie supposed his groups defined by analytic equations. Hilbert's hypotheses are far more general. Without doubt this is still not entirely satisfactory, since though the *form* of the group is supposed any whatever, its *matter*, that is to say, the plane which undergoes the transformations, is still subjected to being a *number-manifold* in Lie's sense. Nevertheless, this is a step in advance, and besides Hilbert analyzes better than any one before him the idea of *number-manifold* and gives outlines which may become the germ of an assumptional theory of analysis situs.

It is impossible not to be struck by the contrast between the point of view here taken by Hilbert and that adopted in his "Festschrift." In this "Festschrift" the continuity assumptions took lowest rank and the great thing was to know what geometry became when they were put aside. Here, on the contrary, continuity is the point of departure and Hilbert is above all anxious to see what we get from continuity alone, joined to the notion of group.

It remains for us to speak of a memoir entitled "Surfaces of Constant Curvature."

We know that Beltrami has shown that there are in ordinary space surfaces which image the non-euclidean plane; these are the surfaces of constant negative curvature; we know what an impulse this discovery gave to the non-euclidean geometry. But is it possible to represent the non-euclidean plane entire on a Beltrami surface without singular point? Hilbert has proved that it is not.

As to the surfaces of constant positive curvature, to which Riemann's geometry corresponds, Hilbert proves that besides the sphere there is no other closed surface of this sort.

(*To be concluded*)

SCIENTIFIC NOTES AND NEWS

Dr. David Starr Jordan has tendered to President Taft his resignation as international commissioner of fisheries, this position having been created three years ago under the treaty of April 11, 1908, with Great Britain. Under the terms of the appointment, the work of the commissioner ceases on the completion of the series of fishery regulations of the boundary waters, and the technical investigations necessary for their completion. This work being finished, the administration of the treaty passes to the Bureau of Fisheries.

Dr. Willy Kukenthal, professor of zoology at Breslau, has been appointed exchange professor at Harvard University during the academic year of 1911–12.

Dr. Edward Miner Gallaudet has retired from the presidency of Gallaudet College, which he has held for fifty-four years.

Dr. Oscar Riddle, of the University of Chicago, has returned from a year of study and travel in Europe. He spent the past six months at the Zoological Station at Naples, whence he now returns to Chicago to take charge of the preparation for publication of the manuscripts left by the late Professor C. O. Whitman. He will also continue certain features of Professor Whitman's investigations.

Professor George E. Sever has been elected president of the Columbia Chapter of

Sigma Xi as successor to Professor William H. Burr.

Dr. F. E. Clements, professor of botany in the University of Minnesota, has been elected president of the Chapter of Sigma Xi in that university.

Mr. W. C. Cox, of the U. S. Forest Service, has been appointed to the newly created post of state forester of Minnesota.

At a meeting of the Royal Dublin Society held on April 25, the Boyle medal of the society was presented to Professor John Joly, F.R.S., whose researches deal with physics, geology, mineralogy, botany and biological theory.

Professor A. C. Seward, F.R.S., has been elected president of a newly established Cambridge University Eugenics Society.

The council of the Institution of Civil Engineers has made the following awards for papers read and discussed during the session 1910–11: Telford gold medals to Mr. W. J. Wilgus (New York) and Mr. J. Walker Smith (Edinburgh); a George Stephenson gold medal to Mr. Philip Dawson (London); Telford premiums to Messrs. G. W. Humphreys (London), H. K. G. Bamber (Greenhithe), A. E. Carey (London), William Dawson (Crewe) and C. S. R. Palmer (London); and the Trevithick premium to Mr. A. T. Blackall (Reading).

Josiah Royce, professor of the history of philosophy at Harvard, will be the university delegate at the celebration of the five hundredth anniversary of the University of St. Andrews.

The American Philosophical Society has made the following appointments of delegates to represent it: At the jubilee of Professor Giovanni Copellini, to be held at Bologna on June 12 next, Professor Dott. Guglielmo Mengairni, of Rome. At the tenth International Congress of Geography to be held at Rome from October 15 to 22, 1911, Mr. Henry G. Bryant. At the thirtieth Congres National des Sociétés Françaises de Géographie to be held at Roubaix, France, from July 29 to August 5, 1911, Mr. Julius F. Sachse.

Mr. C. W. Beebe, curator of ornithology of the N. Y. Zoological Society, and Mrs. Beebe have returned from an extended expedition for the study and collection of pheasants in eastern countries.

It is stated in *Nature* that Professor Hans Meyer will undertake in May his fourth journey in East Africa. Starting from Bukoba, on the west shore of Lake Victoria, he proposes to march to Lake Kiva and the Kirunga group of volcanoes, in order to study the relations of the volcanic phenomena to the tectonic structure of the western rift system at this point. From Kiva the expedition will travel by Lake Tanganyika and, if time permits, also to Lake Nyassa. Besides geological studies, the botany, zoology and ethnology of the region traversed will also be investigated.

On the evening of April 28 Professor Edward L. Nichols, of Cornell University, delivered the annual address before the Iowa Academy of Science on the subject, "The Ends of the Spectrum." Professor Nichols visited the State University of Iowa on April 29, April 30 and May 1. He delivered lectures in the department of physics on recent work in luminescence, and one open to the public on "Daylight."

Dr. John M. Clarke, state geologist and director of science in the New York State Education Department, gave an illustrated lecture before the departments of geology and biology of Colgate University on the evening of May 3. His subject was "The Magdalen Islands and the Bird Rocks."

On April 13, Professor Heinrich Ries, professor of economic geology, Cornell University, lectured at the University of Alabama on the economic geology of the Canadian northwest.

Mr. F. E. Matthes, of the U. S. Geological Survey, is delivering a course of twelve illustrated lectures with accompanying laboratory work before the students of the University of Michigan, the subject of the lectures being, "Topographic Mapping." On May 3, by invitation of the Michigan Chapter of Sigma Xi, Mr. Matthes told in a popular lecture

"How the Map of the Grand Canyon was made." The maps of the Grand Canyon, Yosemite Valley and the new Glacier National Park have all been prepared by Mr. Matthes, who is now engaged upon the map of the new Mt. Ranier National Park.

SAMUEL CALVIN, professor and head of the department of geology, State University of Iowa, and state geologist of Iowa, died at Iowa City on April 17. He was 71 years of age and had been connected with the University of Iowa for thirty-seven years.

PROFESSOR J. BOSSCHA, the Dutch physicist, died on April 15, aged seventy-nine years.

DR. J. T. THOMPSON, the author of valuable contributions to ophthalmology, died at Cardiff on April 28. He was a brother of Professor Sylvanus T. Thompson.

THE directors of the New York Public Library announce a gift of $375,000 by Mr. Andrew Carnegie to be used for establishing and maintaining a library school.

A BILL has been introduced into the legislature of New Jersey providing for the appointment of a state plant pathologist.

DR. CHARLES A. OLIVER, of Philadelphia, has bequeathed his property valued at $15,000 to the Wills Eye Hospital, the University of Pennsylvania and the College of Physicians of Philadelphia.

THE Gray Herbarium, Harvard University, is to have new quarters for its library. The structure will be a two-story addition to the present building and will extend to the west, taking the place of the old library wing, and covering part of the site recently occupied by the Asa Gray House, which was removed some weeks ago. The addition will be of similar construction to the Kidder wing. The library, which will be placed in the new building, is devoted to the classification of flowering plants and ferns. It contains more than 20,000 volumes and pamphlets. The gift which makes possible the erection of the new building amounts to $25,000; it comes from an anonymous friend of the university.

THE research committee of the National Geographic Society of Washington has made an appropriation of $5,000 for continuing the glacier studies of the two previous years in Alaska. The work, beginning in June, 1911, will be done by Professor R. S. Tarr, of Cornell University, and Professor Lawrence Martin, of the University of Wisconsin, who have directed the National Geographic Society's Alaskan expeditions of 1909 and 1910 in the Yakutat Bay, Prince William Sound, and lower Copper River regions. The 1911 expedition will study briefly a number of regions of glaciers not previously investigated by the National Geographic Society, although partially mapped by the Alaska Division of the U. S. Geological Survey, the Boundary Commissions, etc. Work will be done on the present ice tongues and the results of glaciation in the mountains and plateaus of parts of the interior and some of the fiords of southeastern Alaska, the former having lighter rainfall and smaller ice tongues than the Yakutat Bay and Prince William Sound regions.

THE Rome correspondent of the London *Times* calls attention to the fact that for some time past Herr Immanuel Friedländer, of Naples, has been working for the establishment in that city of an International Institute to carry on a continuous and systematic investigation of volcanic phenomena. An observatory has existed on Vesuvius for many years, but from the insufficient means at its disposal no extended and systematic work has hitherto been possible. Such an institute as Herr Friedländer contemplates will be provided with the necessary laboratories and instruments for the regular measurement of temperatures on Vesuvius, for the periodical collection and analysis of the gases, and for the registration and observation of local earthquakes of a volcanic character, not only during the eruptive phases of the volcano, but also throughout its periods of comparative calm. It would form a training school for volcanologists, as well as give opportunity for other scientific persons to make observations. Herr Friedländer's idea is not a new one. An International Institute on Vesuvius was advocated some time ago by Professor Johnston Lavis; Mr. Cool, a

Dutch engineer, and Professor Gaetano Platania have also supported the same idea. In the opinion of Herr Friedländer, however, it would be better to place the institute in Naples itself, where there would be less danger to the costly laboratories and apparatus, and where the vicinity of the university and other scientific institutes would facilitate the work. After communicating his plan to the last International Geological Congress, held in Stockholm, and obtaining the approval of the congress Herr Friedländer set to work to canvass among the scientific societies of every nation for supporters. He has now secured 62 eminent names, among them 25 Italians, 19 Germans and three Englishmen, Sir Archibald Geikie, the president of the Royal Society, Professor H. L. Tempest Anderson, of York and Professor H. L. Bowman, of Oxford. The Royal Academy of Naples and the Geological Committee of Italy have given their adherence to the scheme, some 60 of the most prominent scientific and political personages of Italy are forming a committee to promote it, and the Italian government will shortly decide as to what official support can be given also. Herr Friedländer has himself generously subscribed £4,000 for the building fund, and another £4,000 to be spread over a term of ten years in ten annual payments. The success of the scheme only depends now upon the amount of the subscriptions which will answer Herr Friedländer's appeal to the general public of every country. As far as Italy is concerned the scheme has already obtained the full approval of the most important members of the scientific world.

It is stated in the London *Times* that the regulations issued by the Belgian government for the prevention and cure of sleeping sickness in the Congo provide heavy penalties for neglect of the prescribed precautions. All employers of native labor must take measures to discover any cases of sleeping sickness among their staff and report them at once to the authorities. Those aiding others to neglect the treatment prescribed will be punished; as well as those who try to pass from infected to uninfected districts or *vice versa*. It is noted that in order to combat the disease effectively it is all-important to discover those victims who have not yet reached the second stage—somnolence. Such a measure would tend not only to decrease the mortality but also to limit the dissemination of the germs. All suspects, therefore, are to be examined by the heads of trading posts or sent for inspection to the nearest doctor, who will carry out a thorough examination. Inspection posts are to be established on the main lines of communication in order to prevent suspects from carrying the disease into provinces which are as yet untouched. Natives from the surrounding countries will only be permitted to enter the unaffected regions of the Belgian colony after undergoing a searching medical examination at Ala or Jakoma.

A Conference on Sleeping Sickness has been held at the British Foreign Office as a result of representations made of the danger of the spread of sleeping sickness in consequence of the construction of the Rhodesia-Katanga Railway, which runs from the north of Broken Hill to the Congo frontier and beyond. The delegates to the conference were M. Melot, representing the Belgian government, Dr. van Campenhout, of the Colonial Office in Brussels, Dr. Sheffield Neave, representing the Rhodesia-Katanga Railway, Dr. Aylmer May, representing the Chartered Company, Dr. Bagshawe, of the Sleeping Sickness Bureau, and representatives of the British Foreign and Colonial Offices.

A committee for the study of the sea was appointed in 1909 by the Italian Society for Advancement of Science. *Nature* states that its work was so active and promising that the committee was converted by an act of parliament into an institution of the Italian kingdom. The Regio Comitato Talassografico Italiano is to be concerned with investigations of the Italian seas from the physical and chemical points of view as well as from the biological. Great importance will be attached to practical questions concerning the navigation and the fisheries. Investigations of the

high atmosphere will also be made in connection with aviation. The president of the committee is the marine minister, and representatives of the chief institutes, academies and societies which take interest in sea investigations have been appointed as members. In addition the committee has a scientific staff of its own; it receives a yearly grant from the Italian government of 60,000 lira; and the ships for the cruises are supplied by the Italian navy. Four cruises in the Adriatic Sea have taken place already, the program of which was agreed upon with the delegates of the Austrian government, and a fifth cruise will soon start.

THE report of the departmental committee appointed to report on the present condition and the future development of the collections comprised in the Science Museum at South Kensington and the Geological Museum in Jermyn-street, has been issued as a parliamentary white paper. According to an abstract in the London *Times* the committee finds that the objects now exhibited are so much crowded that their due classification and utilization are impossible. Buildings twice the size of those now used would be fully utilized by the existing collections without the addition of a single specimen. The committee states that the physics section is hopelessly overcrowded. In the motor car and aeronautical groups, both early construction and later developments will require further illustration. The electrical engineering section requires to be increased by five or six times its present dimensions. In no section is there more urgent need of early action to secure for the museum examples of instruments and appliances that have marked the opening of a new era in invention and industry. A conference room, where scientific or technical societies might meet, a large lecture theatre, public demonstrations in the galleries, and the exhibition of temporary collections are also suggested. It is recommended that the geological survey offices and library and the Museum of Practical Geology, which are now cramped by the limitations of the building in Jermyn-street, should be grouped, as at present, in a single building, and it would be of great advantage to have that building erected as part of the general scheme at South Kensington. If the collections in the Science Museum and in the Jermyn-street Museum were brought together they would provide the basis of a collection that would be complete as regards stratigraphical and economic geology. Such a collection in the new buildings, with the systematic collection of minerals and the paleontological collections in the British Museum (Natural History), would represent at a single center the whole field of geological science. In most of the departments of science and its applications, the committee concludes, the museums contain much that is of great historical interest and value. They are rich in specimens, instruments, machines and models selected and exhibited in such a manner as to repay systematic examination by the student. In many sections, however, the collections are now far below the standard which it is clear they ought to reach in these matters, and their proper organization is impossible in the existing accommodation. A science museum in which all branches of physical science, pure and applied, and the scientific and economic work of the geological survey shall be adequately illustrated in close proximity to the other great museums at South Kensington would be of incalculable benefit alike to intellectual progress and to industrial development.

UNIVERSITY AND EDUCATIONAL NEWS

AT the recent session of the Alabama legislature the University of Alabama was given an additional appropriation of $300,000, to be expended during the next quadrennium for maintenance and new buildings.

Two gifts from Mr. Carnegie to the Carnegie Technical Schools were announced last week. On his recent visit to Pittsburgh he presented the schools with a valuable 725-acre tract of land that he had owned for some years at Garver's Ferry, twenty-five miles up the Allegheny River from Pittsburgh. It will be converted at once into an experimental

station and engineering camp. The other gift was a set of designs by Mr. John Wynkoop, made in the École des Beaux Arts of Paris and awarded a medal.

The inauguration of Dr. George Edgar Vincent as president of the University of Minnesota will take place October 18 or 19 next. The date has been fixed by the fact that the American Association of State Universities will meet at Minnesota on these days.

Professor James R. Angell, head of the department of psychology and dean of the Senior Colleges, has been chosen by the board of trustees of the University of Chicago to succeed George E. Vincent, now president of the University of Minnesota, as dean of the faculties of arts, literature and science.

Mr. George Chandler Whipple, formerly in charge of the biological laboratory of the Boston water department and later of the sanitary work connected with the water supplies of New York City, since 1904 practising sanitary engineer, has been appointed professor of sanitary engineering in the Graduate School of Applied Science of Harvard University.

Dr. Ernest Sachs, of New York City, has been appointed associate in surgery at the Washington University Medical School, St. Louis.

In Stanford University J. A. Koontz and E. G. McCann have been made instructors in electrical engineering.

Dr. H. N. Alcock, London, has been appointed to the chair of physiology in McGill University.

Dr. Emil Abderhalden, professor of physiology in the Berlin veterinary school, has been called to Halle, to succeed Professor Bernstein, who retires from active service at the close of the present semester.

DISCUSSION AND CORRESPONDENCE

THE COMPARATIVE VALUE OF METHODS FOR ESTIMATING FAME

In a recent contribution upon "Historiometry as an Exact Science"[1] Dr. F. A. Woods calls attention to what appears to be a failure of the "space method," as compared with the "adjective method," in solving the problem which I proposed in Science, October 7, 1910, viz., to determine by purely objective methods the comparative fame of Sophocles and Euripides. This apparent failure might seem to support my statement that "historiometry so-called can never aspire to the name of an exact science" were it not for the fact that Dr. Woods has not established the superiority of the adjective method in this particular instance. For the purpose of illustrating the comparative value of methods for estimating fame I wish to examine the problem of the two Greek poets a little more closely.

Those who are familiar with Greek literature are well aware that Sophocles is superior to Euripides in majesty, grandeur and the various other qualities quoted by Dr. Woods from Mr. Jebb and the critics. But there was one quality, not named by Dr. Woods, in which Euripides excelled Sophocles and this one quality more than outweighs the sum of his deficiencies. Mrs. Browning alludes to this quality in her poem "Wine of Cyprus."

> Our Euripides the human,
> With his droppings of warm tears,
> And his touches of things common
> Till they rose to touch the spheres.

The humanity of Euripides and "his touches of things common" have appealed to mankind far more than the majesty and ideal art of Sophocles. Aristotle states that Sophocles represented the men and women of his dramas as they ought to be, but that Euripides represented them as they actually were. It was because he was the first to portray upon the stage the motives and lessons of every-day life that philosophers, statesmen, poets and all other conditions of men have come to prefer the plays of Euripides to those of any other ancient writer.

In comparing Sophocles and Euripides it must be remembered that the latter inaugurated a new epoch and the changes which he

[1] Science, April 14, 1911.

introduced into the drama found disfavor among the Athenians of the old conservative school. It was for this reason that Sophocles won five times as many prizes as his younger rival; yet Sophocles himself came to see the significance of the new movement and in his later years began to imitate Euripides.

As an influence in human history Sophocles almost sinks into insignificance when compared with Euripides. Historians dwell at great length upon this point. Curtius in speaking of "this importance of Euripides for the general history of the world" makes the following statement.

The real classics, such as Pindar, Æschylus and Sophocles, are only to be thoroughly understood and appreciated by contemporaries, or by those who by study accommodate to them their whole way of thinking. Euripides, on the other hand, by the very circumstance that he put an end to the severe style of earlier art, stepped forth from the narrower sphere of the merely popular; he asserted the purely human motives of feeling which find a response in every breast, hence his clearness and intelligibility; hence without presuming any special interest in the subjects derived from mythology or claiming a higher strain upon the intellectual powers, he satisfies the demands which men at all times and in all places make upon the drama. He is at once interesting and entertaining, terrific and affecting; he offers a wealth of thoughts and reflections, which come home and are of importance to every one, and is a poet for every educated man who understands the language in which he writes. For the same reason, too, he was able to affect the minds of the foremost among his contemporaries, such as Socrates; and the language of the Attic stage, as he developed it, became the standard for the drama. For the same reason he also pointed out its path to plastic art, and showed it how it could do new and important things after the age of Phidias; and therefore, though in his lifetime he had been unable to prevail against the still acknowledged tradition of earlier art, he filled the world with his fame after his death, and found numerous followers among the poets, who made use of the Greek myths in order to obtain dramatic effects of universal human significance.

This passage from Curtius is of great interest, for it not only illustrates the greater historical importance of Euripides, but it also shows that the ultimate significance of a man's work can not be measured by the prizes or honors which he may receive from contemporaries and that the forces which bring a man fame may go on with far greater intensity after his death than during his life time.

In order to illustrate what the historian means when he says that Euripides "satisfies the demands which men at all times and at all places make upon the drama" a few examples may be given.

Curtius states that the plays of Euripides accompanied the Athenian traveler by land and sea; so also in modern times when De Quincey started on his wanderings he took with him a pocket volume of Euripides. Even Mr. Roosevelt, when preparing for his African hunting trip, included in his famous "pigskin library" a copy of this same poet.

Lucretius in discussing the indestructibility of matter translates from Euripides, "Nothing that exists can perish; but everything on decomposing takes on a different form"; so also in modern times von Lippmann, in the introduction of his "Abhandlungen und Vorträge," hopes that the reader may imbibe the spirit of Euripides, who said, "Happy the man who has gained a knowledge of science."

The Greek poet Ion in his elegy to Euripides reminds him that his fame will endure as long as Homer's; and Dante in his "Divine Comedy" mentions among the shades of departed Greek poets Homer first and then Euripides. Dante does not speak of Sophocles in his whole poem, and we can see from this how slight the influence of Sophocles was upon the thought of the middle ages.

Seyffert in his "Kulturgeschichte der Griechen und Römer," when discussing the development of the drama, states that "the tragedians following Euripides made him their model and pattern without qualification and the Roman poets preferred paraphrasing his dramas to those of other tragedians." The Roman poet Ennius paraphrased the "Andromeda" and some twenty other tragedies of Euripides; so also we find in more modern times that Racine paraphrases the "Andromache" and other plays, Goethe paraphrases

the "Iphigenia," and Browning the "Alcestis." Racine, Goethe and Browning selected Euripides and not Sophocles for their special purposes, owing to the fact so well stated by Perrin that Euripides comes nearer to the modern heart than Sophocles or any other ancient poet. The best testimony upon this point, however, is that of Racine himself, who, writing in 1676 in the preface to his "Iphigenia," expresses his indebtedness to Euripides as follows:

> As regards the portrayal of the passions I have endeavored to follow Euripides most exactly. I confess that I owe to him a large number of the passages which have been most praised in my tragedy. I have seen with pleasure, from the effect which my imitations of Homer and Euripides have produced upon our audiences, that good sense and judgment are the same in all ages. The taste of Paris conforms to that of Athens. My audiences have been moved by the same things which once moved to tears the most intelligent people of Greece and which made them say that among the poets Euripides was the most tragic of all; that is to say he knew how to excite to a marvellous degree the feelings of pity and fear, which are the true ends of tragedy.

It is probable that Euripides through his "Iphigenia" alone has exerted a greater influence upon modern thought and feeling than Sophocles with all his plays combined. Erasmus in 1524 translated the "Iphigenia" from Greek into Latin; Dolce gave an Italian rendering in 1560; Sibilet (1549), Rotrou (1640), Racine (1674), Leclerc and Coras (1675) gave different French imitations; many English versions were given in the eighteenth century; Goethe's "Iphigenia" was completed in 1787; Gluck's opera upon the "Iphigenia" was produced in 1774 and since his time over twenty other composers have set music to the same theme. The recent revival of interest in the "Iphigenia" through the choral dances of Miss Duncan is well shown by the increased demand for this and other plays of Euripides at book stores and libraries.

Many other examples might be given to illustrate the much greater historical importance of Euripides as compared with Sophocles, but enough has been produced to show that as regards the special purposes for which mankind at large read, consult, quote, paraphrase or otherwise make use of a poet Euripides has always been preferred to Sophocles. And the approximate ratio of this preference, according to the five objective methods employed in my previous paper, is over 2:1.

The failure of the adjective method to give a verdict agreeing with that so unmistakably expressed by history and by mankind at large is very evident. The adjective method—by which is meant the ratio of the number of adjectives of praise against those of dispraise—neglects to give the specific value of the terms, human, sublime, artistic, etc., the summation of which is supposed to constitute fame. The ratio of mere numbers gives each qualifying adjective the same value, when perhaps the number of adjectives expressing humanity and feeling should be raised to the tenth power and those expressing majesty and art only to the second power.

The mathematical formula for expressing fame (F) in the terms of its components a, b, c, etc., is not $F = a + b + c \cdots$, but $F = x \cdot a + y \cdot b + z \cdot c \cdots$, in which x, y, z, etc., are unknown and indeterminate functions. That historiometry can never become an exact science is evident from the fact that the values which men give these unknown historiometric functions are different in different ages, races and individuals. The twentieth-century mind would lay more stress upon the scientific, the medieval mind upon the mystical; the Roman would lay more stress upon the legal, the Greek upon the beautiful; the clergyman would lay more stress upon the ideal, the business man upon the practical. Until historiometry can develop a set of functions whose values shall be constant for all men in all ages it must remain among the most inexact of sciences.

Another objection to the adjective method is that fame is not a mere summation of eulogistic attributes. Napoleon, for example, heads Professor Cattell's well-known list[2] of 1,000 eminent men, in connection with which list its author makes the following statement:

[2] *Pop. Science Monthly*, February, 1903, p. 362.

"There is no doubt but that Napoleon is the most eminent man who has ever lived, yet it should give us pause to think that this Titan of Anarchy stands first in the thoughts of most men." In the passage just quoted we have one extremely eulogistic phrase "most eminent man" counterbalanced by another phrase of extreme disparagement "Titan of Anarchy." A similar array of favorable and unfavorable expressions can be found in any impartial biography of Napoleon. It is this peculiar blending in one man of different extremes which has given Napoleon and many other men a great share of their celebrity; in such cases the ratio of the numbers of adjectives of praise against those of dispraise fails to give a true answer to the question, which man of a given group of men is the most eminent or historically most important.

The space method and reference frequency methods of estimating fame are not open to the objections which have been raised against the adjective method. The historian in discussing, for example, the respective influence of Euripides and Sophocles upon human affairs must necessarily devote more space and make more references to Euripides since his influence in this respect was much the greater, yet in doing this he need not necessarily employ any adjectives of direct praise or dispraise.

The space method and reference frequency methods are also more free from the errors of personal equation than the adjective method. In the sentence "Cæsar was ambitious" one person might regard ambitious as a term of praise and another of dispraise, yet these two persons would agree perfectly as to the number of lines in a biographical sketch of Cæsar or as to the number of times Cæsar was referred to in an index.

In the selection of a method for estimating historical values it would seem then necessary first of all to dissociate the question of merit from that of fame, and the questions of excellence in particular directions from the broader questions of historical importance. For estimating merit and excellence in particular qualities, which is perhaps the chief concern of the critic, the adjective method proposed by Dr. Woods may possess certain advantages. But for estimating fame and historical importance, which is the chief concern of the "historiometrician," the adjective method would seem far inferior to the space and reference frequency methods.

As to the exactness of historiometry as a science, may we not say what Huxley once said of another science, the most exact of all. It "grinds your stuff of any degree of fineness, but nevertheless what you get out depends on what you put in."

C. A. BROWNE

NEW YORK CITY

DR. WOODS'S APPLICATION OF THE HISTOMETRIC METHOD

THE paper by Dr. F. A. Woods, published in SCIENCE, April 14, giving the results of his metrical investigation of the biographies of eminent Americans is one of great interest. Both in method and results it opens fields of investigation of the highest sociological value. He has proved the reliability of his figures by reaching approximately the same results, for the state of Massachusetts and the other thirteen original states, when using different sets of data; and while the variation in the results indicate what would be considered in physics as a large probable error, yet they are really small considering the method used and the number of observations.

If the wide range shown thus by the different states in their production of eminent persons per thousand of their white population can not be explained by environment it is evident that the arguments for the dominance of hereditary ability will be strongly supported. On the other hand, if it can be explained by a high coefficient of skew correlation with one or more series of quantities expressing any antecedent social condition it leaves just so much less for heredity to explain. Thanks to the work of Galton and others, heredity is already mathematically expressed by the correlation of the characters of individuals in successive generations. And perhaps for that reason the tendency now is to exaggerate the

relative importance of heredity. Dr. Woods himself, judging from his article in *The Popular Science Monthly*, April, 1910, has taken an extreme view of the dominance of heredity over environment, and apparently he expects the present investigation to support that view. The further publication of his results will therefore be awaited with special interest.

The first series of quantities expressing environment in youth which is suggested for correlation with the percentage of eminence in maturity is educational. The simplest measure of educational opportunity in each state is its expenditure per capita for school purposes. We do not know the average per annum, but we do know, accurately for most of the states, the public expenditure forty years ago for the education of the present generation. These data are given in Table 14 of the Report of the Commissioner of Education for the year 1910. For a first approximation we can assume that the ratio of per capita expense in the different states has been constant, and it is only with the ratio that we are concerned.

Massachusetts stands highest with $3.73 per capita, which was more than twice the average of the whole country, and also more than twice the average of the thirteen states Dr. Woods has tabulated. Connecticut is second with $2.74. New Hampshire stands near the average. But it is well known that private educational institutions are relatively more important in New England than in any other part of the country; while Harvard and Yale colleges would substantially raise the pre-eminence of Massachusetts, Connecticut, and the whole group, in the total per capita expense table, and so give them the places they have in Dr. Woods's table.

Virginia, which Dr. Woods found to be generally below the average in the table of eminence, is credited with only 47 cents in the expense table. But including West Virginia, which was not separated until 1863, the average was 86 cents.

South Carolina, which Dr. Woods found to be slightly above the average for the white population, is credited with only 38 cents per capita of the whole population. But that state had the highest percentage of colored population (60 per cent. in 1870), while forty years ago most of the school money was spent on the white schools, and in ante-bellum days of course it was all for the white population. This would raise the per capita expense to 95 cents, and give it good rank among the southern states, but still below the average of the thirteen states. Why this state should stand above the average in Dr. Woods's table remains to be explained. It shows, as we should expect, that schools do not supply all of a child's environment, and other correlations must be sought—the ratio of whose coefficients ought to be significant of their relative importance in making eminent persons.

North Carolina, which Dr. Woods found "has always had the worst record" for the production of eminence, the percentage being about one-fourth the average, is credited with only 16 cents per capita for education—this being the lowest for the thirteen states, and also for the whole country—being in fact about one tenth the average. But applying the same total expenditure to the white population the per capita expense would be 25 cents—about one sixth the average.

The average number of days of public schooling given to each inhabitant between the ages of five and eighteen years, in the school year 1870–71, in the New England and middle states was 70.2 days, and in the southern Atlantic states only 18.1 days. The per capita expense therefor was $2.38 and 63 cents, respectively.

We seem to be near the time when the cost of encyclopedic eminence can be stated in dollars without making any reflection on the compilers of those works.

Further, if we can approximate the expense of higher as well as elementary education in each state, we can easily, by the method of least squares, determine the ratio between them which is most favorable for the production of eminent persons.

It appears that Dr. Woods has directly opened the way to the mathematical determination of the relative importance of hered-

ity and environment. At least we may expect a flood of new light on the subject from histometrical investigations, and if the conclusion is different from what the author of the method anticipated it will not at all detract from the credit due him for its development.

Geo. H. Johnson

Brooklyn, N. Y.

METALS ON METALS, WET

To the Editor of Science: A year or two ago I repeated to a class in elementary physics the statement, familiar to generations of engineers on the authority of General Morin, supported by the approval of Rankine, that the coefficient of friction of *metals on metals, wet,* is considerably greater than that of *metals on metals, dry.*

Thereupon a thoughtful youth in the class asked me why, if this were the case, it was customary to put sand on wet car-rails to prevent the slipping of the driving-wheels. Taken aback by this unexpected scepticism, I begged for time to find the right answer to the disturbing question and set to work experimentally on the problem. The student reported after a time that trackmen had told him the water they had trouble with was usually slimy, which seemed to be a fairly satisfactory explanation of the puzzle; but meanwhile my experiments had shown some interesting facts, which I will here set forth.

Using a disk of brass, about 7.5 cm. in diameter and about 0.6 cm. thick, on a flat brass plate, I found:

1. That, when there was no load on the disk, a few small drops of water placed between it and the plate multiplied by a factor which might be as great as 3 the friction between the two.

2. That, when the disk was heavily loaded, the presence of the few small drops of water between it and the plate made little, if any, relative difference in the friction between the two.

3. That when plenty of water was used, so that it covered the whole space beneath the disk and extended somewhat beyond the edge, the disk without load was drawn along the plate quite as easily, apparently, as when both were dry.

From these facts I came to the conclusion that the increase of friction observed in case 1 was not due to an increase in the *coefficient* of friction caused by the water, but merely to the increase of pressure between the disk and the plate, caused by the suction of the capillary perimeters of the water-spots between them. When there is much water, its perimeter is outside the edge of the disk, is wide, or thick, and has little effect.

Some little search in books dealing with the subject of friction has failed to show there any recognition of this possible explanation, and refutation, of the Morin-Rankine statement of the large value of the coefficient for *metals on metals, wet;* but I should hardly have written you about the matter if I had not recently found this statement repeated in the "Smithsonian Physical Tables" published in 1903. I hope the new edition of these tables will not quote Rankine on this particular without further evidence.

Edwin H. Hall

Cambridge, Mass.,
April 29, 1911

SCIENTIFIC BOOKS

The Stability of Truth. By David Starr Jordan. New York, Henry Holt & Co. 1911. Pp. 180.

"This little book," says the author, "represents the substance of a course of lectures delivered on the John Calvin McNair foundation in the University of North Carolina, January, 1910."

The chapter headings are: Reality and Science, Reality and the Conduct of Life, Reality and Monoism, Reality and Illusion, Reality and Education, Reality and Tradition.

Evidently something has happened in philosophy, in science or in both when a scientist of the first class, not to say the author of this volume in particular, puts out a book with the good old philosophical term, "Reality" at the head of every chapter. Doubtless in the minds of most scientists there will be little question about where "something has happened."

They will say that philosophy has at last discovered that neither its problems nor its methods are so fundamentally different from those of science as was once supposed; that there is now a great philosophical movement, with an evolutionary logic—the conception of the working hypothesis—as its avowed method; and that it is therefore now possible for a scientist to stroll into the field of philosophy and set to work with his own tools. And indeed the spirit of this philosophical movement variously known as "pragmatism," "evolutionism" and "experimentalism," is certainly very different from the Hegelian idealism, which found the difference between philosophy and science to consist in the fact that the doctrines of the former are "necessary," while those of science are merely "hypothetical."

Still, in such a *rapprochement* as is taking place between philosophy and science it would be strange if all the change were on one side. For one thing, it seems obvious that the surrender of absolutistic methods by philosophy means added responsibilities for scientific method. Under the old régime science, even while renouncing and denouncing philosophy and all its works, found comfort in turning over to philosophy certain ethical and social questions which it found difficult to handle or which interfered with the pursuit of "purely scientific truth."

Much of the doctrine of this book (whose title, by the way, means that the only truth that is stable is that truth is not stable) is to the effect that if important human interests formerly turned over to transcendental methods are now thrown back upon scientific method, this method must be human enough to take care of them. And this means that scientific method and interest can not be purely intellectualistic. The author says:

> The purpose of this book is to set forth the doctrine that the final test of truth is found in trusting our lives to it. . . . The primal impulse, as well as the final purpose of science is the conduct of life. . . . Pure science can not be separated from applied science. . . . Knowledge is power; power is evidence that our belief is knowledge.

These and other similar statements on almost any page warrant the reader in saying that the book points at the hyper-intellectualism of science no less than at that of philosophy.

From the specific doctrines of the book the following points have been specially noted: (1) The recognition, rather unusual in a natural scientist, of the social character of consciousness and the self. "I think, therefore I am, gives place to we think, therefore we are." (2) Mr. Balfour's philosophic doubt is well hit off as "a process by which men question the only things they know to be true in order to prove the reality of things they know not to be true." This applies to all "transcendental doubt." (3) The "recrudescence of superstition," which accompanies an age of science, "is made possible by the fact that the traditions of science are so diffused in the community at large that fools find it safe to defy them." (4) Superstition and dogmatism are shown to be identical in so far as both ignore the process of experimentation. (5) The chapter on Reality and Monism, which is one of the best, puts two questions to Haeckel's monism: Is it a genuine scientific hypothesis, that is, one capable of verification? Is it of any ethical significance in the conduct of life? The author finds for the negative in both cases. (6) In the last chapter on Reality and Tradition, the "warfare between science and theology" is found to be quite as much a warfare between old and new science and to exist in the individual mind of the scientist and theologian alike.

In view of the general insistence upon the organic connection between science and the conduct of life, some readers may find difficulty with certain passages on "Belief," in which belief is justified as a "philosophical" category by "its effect on the conduct of life," even though it "is not reducible to terms of human experience" (pp. 42, 44). But "as men of science," we can not accept any hypothetic "articles of faith" not resting on "scientific induction." "I ought not to say I believe when I can not say I know" (p.

88). Some may take this to mean that "the conduct of life" may still be considered (*e. g.*, ethically or "philosophically") apart from science, and, conversely, that science may still have an aspect (*e. g.*, the pursuit of truth) that is independent of the conduct of life.

The author's captivating style is too well known to call for comment. The publishers have given the book a very attractive form.

A. W. MOORE

Allen's Commercial Organic Analysis. Volume IV., Resins, India-rubber, Gutta-percha and Essential Oils. Philadelphia, P. Blakiston's Son and Co. Pp. viii + 466. $5.00 net.

The subjects covered in this volume are: Resins, by M. Bennett Blackler; India-rubber, Rubber Substitutes and Gutta-Percha, by E. W. Lewis; Hydrocarbons of Essential Oils, by T. Martin Lowry; Ketones of Essential Oils, by T. Martin Lowry; Volatile or Essential Oils, by Ernest C. Parry; Special Characters of Essential Oils, by Henry Leffmann and Charles H. LaWall.

As with the previous volumes of the series, the book contains a very large amount of detailed information which is very valuable for any one who has occasion to work with the great number of organic compounds which are used in industry. The preparation of the successive chapters by chemists who have expert knowledge of the subjects of which they write insures accuracy and a wealth of information which it would be impossible to secure in any other way.

W. A. NOYES

SCIENTIFIC JOURNALS AND ARTICLES

THE April number (volume 12, number 2) of the *Transactions of the American Mathematical Society* contains the following papers:

Anna J. Pell: "Biorthogonal systems of functions."

Anna J. Pell: "Applications of biorthogonal systems of functions to the theory of integral equations."

C. N. Moore: "On the uniform convergence of the developments in Bessel functions."

H. H. Mitchell: "Determination of the ordinary and modular ternary linear groups."

G. D. Birkhoff: "General theory of linear difference equations."

THE April number (volume 17, number 7) of the *Bulletin of the American Mathematical Society* contains: "Groups generated by two operators satisfying two conditions," by G. A. Miller; "Fundamental regions for cyclical groups of linear fractional transformations on two complex variables," by J. W. Young; "On the relative discriminant of a certain Kummer field," by Jacob Westlund; "Note on reciprocal figures in space," by Peter Field; "Mathematical physics for engineers," review of Gans' Einführung in die Theorie des Magnetismus, Schaefer's Einführung in die Maxwellsche Theorie, and Jahnke and Emde's Funktionentafeln und Curven, by E. B. Wilson; "Shorter Notices": Huntington's Fundamental Laws of Addition and Multiplication in Elementary Algebra, by N. J. Lennes; Borel's Théorie de la Croissance, by R. D. Carmichael; Tannery's Elemente der Mathematik, by J. B. Shaw; Weitzenböck's Komplex-Symbolik, by C. L. E. Moore; Staude's Analytische Geometrie des Punktepaares, des Kegelschnittes und der Fläche zweiter Ordnung, by D. D. Leib; Festschrift zur Feier des 100 Geburtstages Edouard Kummers, by L. E. Dickson; Thiele's Interpolationsrechnung, by H. L. Rietz; Slaught and Lennes's Plane Geometry, by F. W. Owens; Breckenridge, Mersereau and Moore's Shop Problems in Mathematics and Lester's Integrals of Mechanics, by C. F. Craig; Annuaire du Bureau des Longitudes, by E. W. Brown; De Montessus' Leçons élémentaires sur le Calcul des Probabilités, by E. B. Wilson; "Notes"; "New Publications."

The May number of the *Bulletin* contains: Report of the February meeting of the society, by F. N. Cole; "On the classification of crystals," by Paul Saurel; "Horner's method of approximation anticipated by Ruffini," by Florian Cajori; Review of the New Haven Colloquium Lectures, by G. D. Birkhoff; "Shorter Notices": Bauer's Vorlesungen

über Algebra, by Arnold Dresden; Richard and Petit's Théorie mathématique des Assurances, by E. B. Wilson; "Notes"; "New Publications."

THE QUIZ DEMONSTRATION SYSTEM OF TEACHING QUALITATIVE ANALYSIS

It is time that we are awakening to the fact, that in the line of elementary laboratory work there is altogether too much poor impartation of knowledge. This is particularly the case as regards general chemistry and qualitative analysis, especially when considered from the standpoint of those who expect to carry on their life work in the field of engineering and industrial chemistry.

Qualitative analysis is especially subject to error. Who would think of trusting to a civil engineer, ignorant of the strength of the material employed, the construction of a bridge? Or one's child to a doctor, if cognizant of the fact that he did not know the properties of the drug he was administering? Is it not of as great import to a chemist that he understand the properties of the chemical elements, the material he is using in his daily work?

This being true, why is it that the laboratory instruction is, in many cases, left to assistants paid the munificent sum of from $200 to $500 per year, with, consequently, very indifferent instruction? If they are the best the institution can afford, the fault can be remedied, in part, by the man in charge giving to his assistants all the instruction within his power.

Detail laboratory instruction is the hardest of work, if rightly given, as difficult as any quiz or demonstration, for what is it, if properly conducted, but one continual individual quiz and demonstration of from two to three hours' duration? It is common to consider from two to three laboratory hours as equivalent to one lecture or quiz hour. This is a mistake, at least as far as the instructor is concerned, for it is not a greater impossibility for a man to lecture or quiz for half a day at a time, day in and day out, than for him to give the best that is in him to a laboratory class extending over a like period. I hear some one reply that it takes more time to prepare for a lecture or quiz than for a laboratory period. Granted, when the laboratory work is conducted as is most customary. But when the instructor keeps abreast of the times, makes a thorough test of the new methods, keeps track of and endeavors to overcome the difficulties of the ordinary class in qualitative analysis, he will devote much more time to the preparation of his work than a language teacher, for instance, who, year after year, employs the same text in class work. You see I am not making the statement, "He does this," but that he should. This is, of course, not possible when the hours of labor are too many to allow for it the requisite time. They should be shortened. There should be a certain amount of time spent by the instructor in his laboratory "doing things." A German teacher must know how to read the language. To teach laboratory work correctly a man must be able to do the work well himself.

Inasmuch as I am desirous of suggestions and criticisms from my fellow instructors, an underlying, selfish motive prompts me to present the scheme for laboratory instruction employed by me. It is one which I have successfully made use of during the past three years and I feel it a step in advance of the methods heretofore used by me, and of those which I have seen employed elsewhere.

Chemical theory is based on facts obtained in the laboratory. It is then true, that for a thorough comprehension of the theory it is necessary that the student be conversant with the facts before he can understand the application. I proceed, therefore, with this in view, as my main objective point.

As an essential, the instructor must see every test made by the student. That this may be accomplished the too often "drifting" about the laboratory by the instructor must be done away with. There must be system. There must be known, to some one in charge, what is going on in every part of the laboratory. Yet in this system, two things must be guarded against in the student. First, lack of independence. Second, useless waste of time,

with consequent disheartenment and lack of interest in the work.

In considering Group I., consisting of lead, silver and mercury, the student is given a typewritten sheet of reactions to perform. These are carefully selected to bring out the properties of the elements, especially those which are of most importance in qualitative and quantitative analysis. At the completion of the experiments on silver or on silver and lead, in place of throwing out the contents of his test-tubes, the student takes them to the instructor in charge, and the latter, a man of experience, after examining the work carefully, gives him a thorough quiz on it. If this work, as well as the student's knowledge of the reactions, etc., is satisfactory, the instructor places his O. K. upon the sheet. If neither the quiz has been passed in a creditable manner nor the student been able to obtain the correct reactions, the contents of the test-tubes are thrown out and the work repeated. After a second trial, if the experiment is still unsuccessful, the instructor should demonstrate it. By this means the student is taught independence, there is created in him ability to do things for himself; the instructor is enabled to "keep tab" on the work accomplished, knows if the student has obtained the correct result and yet does not allow him to spend an undue amount of time on something which it is clear he does not understand.

This quiz demonstration system is varied to suit the needs of the individual. These, the instructor, coming in personal contact with each student as he does, soon comes to know.

Before the younger men of the force are allowed to quiz, they should observe the methods of the instructor in charge and then be subject to his direct supervision from time to time, when they themselves are quizzing. Thus, in as far as is possible, the policy of the laboratory is uniform and at the same time consistent with the individuality of the persons in question.

Upon completion of the preliminary work upon a particular group, the separation of the included elements is studied in a similar manner. When the results of this work have received the O. K. of the instructor the student is given a number of simple unknowns on this group. I find that by requiring that he do two of these for every one upon which he makes a mistake, his mistakes become fewer in number and his confidence in his ability thereby increases. In taking up the second and following groups, the separations and preliminary work are treated similarly to group one, except that when time is available one of the unknowns is made to contain one or more of the elements of the preceding group. In this way the separation of the various groups may be quizzed over as in the separation of each group.

The groups and acids take up somewhat over one half the time allotted to the course. Then come the general unknowns, where the work of the student is expected to be carried on independently. Several simple mixtures are first given, that the student may better connect the group separations. In these both the metal and acid are identified. Then follow a number of commercial products, consisting of minerals, slags, alloys, etc., the selection being governed, to a certain extent, by the particular field which the student is likely to enter. In this district mining interests are of most importance, hence, ores, minerals, slags, etc., make up this portion of the course.

Individual quizzing on this part of the work is not as frequent as before, but when reporting an unknown, the student is quizzed on reactions, method of separation, identification, etc. Here class-room quizzing is of more value and can be advantageously used to supplement part of the work with the individual, for, provided he be properly trained in the power of observation, a great amount of knowledge can be gleaned by the student from the mistakes of others, brought to his notice by this group questioning.

At this point too much stress can not be laid upon the exceptions to the general rules and the reason for each step taken in the separation. Herein is differentiated the training of the professional from the routine chemist. I find, also, at this point in the training, of the

utmost importance, and yet one of the greatest difficulties encountered, is the mastery of the proper method of disintegration, especially of the insoluble substances and those which are likely to lose part of their content by volatilization. If a proper solution of the unknown is obtained, the analysis is comparatively easy, whereas, if not obtained, incorrect results are sure to follow. Alloys and metallurgical products containing relatively small amounts of some one substance also require special attention.

Objections have been raised to the use of technical products for unknowns, claiming that they do not give the proper amount of training. This is apt to be true where unknowns of a commercial nature are taken just as they come to hand without special effort on the part of the instructor. It is certainly not the case if care is taken in obtaining what is necessary to suit the problem in question, for there are certainly sufficient varieties of commercial products to cover the field. Aside from giving the students a training not to be had in the use of laboratory prepared unknowns, his interest is much more easily aroused and held when he can see something "practical" in what he is doing.

I have found that where lectures are combined not alone with class-room quizzing but as well with this demonstration method the student is made to think and gets a grasp on the subject well worth the time spent in its acquisition.

Raymond C. Benner
University of Arizona,
Tucson, Ariz.

HUMUS IN DRY-LAND FARMING

It has been the consensus of agricultural opinion and experience, both in this country and in Europe, that the production of wheat on the same land year after year results in steadily decreasing yields. Chemical investigations in several instances have shown this decrease in yield to be accompanied by a correlated decrease in the supply of humus and of nitrogen in the soil. Under the title of "The Nitrogen and Humus Problem in Dry-Land Farming," Mr. Robert Stewart, chemist of the Utah State Experiment Station, has recently published the results of some investigations with special reference to the effect of continued wheat growing on the non-irrigated lands of the Cache Valley in Utah.[1]

Mr. Stewart's investigations in the Cache Valley indicate that the continuous production of wheat in that section has not resulted in a reduction of either the humus or the nitrogen supply of the soil, at least during the thirty years or more that wheat has been so grown there. He finds, indeed, that in something over twenty cases where comparisons were possible between virgin soil and soil that had been cropped to wheat for several years there has been a slight increase, both in the total nitrogen and the humus in the surface foot. In the second foot of soil on these two sets of fields he finds a decrease of the total nitrogen on the cropped land, but a marked increase in the humus. His summary of results shows that on the wheat land there has been a 10 per cent. increase in the humus supply of the surface foot and a 25 per cent. increase in the second foot.

Mr. Stewart wisely avoids any generalizations upon the limited data he presents in this publication. But it is unfortunate that he does not give more consideration to the agricultural conditions and farming methods that prevail in the region of which he writes. Unless the reader of Mr. Stewart's bulletin is familiar with conditions in the Cache Valley, the results presented are likely to seem either pointless or irreconcilable with the results of similar investigations elsewhere. To one who knows those conditions, the brief statement that "Some of the farms of this district have been under cultivation for forty-five years, and apparently yield as good crops as they ever did" may seem to be a good and sufficient epitome of the situation; but if one does not know the region, this sentence hardly seems adequate.

It is true that accurate data as to the farm yields for past years are difficult to obtain and

[1] Utah Agricultural College Experiment Station Bulletin, No. 109, August, 1910.

are unsatisfactory to use, because of the uncertain factor of variable climatic conditions from year to year; but some comparisons might have been made between the yields obtained during recent years from land that has long grown wheat and the yields on virgin or nearly new fields on similar soils. Or, lacking such data, it would have been helpful to the reader had there been given some statement as to the present wheat yielding capacity of the fields from which the samples were obtained.

Unless it is shown definitely that the maintenance of the nitrogen and humus content of these Cache Valley soils is correlated with the maintenance of their wheat yielding capacity, these investigations lose much of their possible value.

As to the matter of the farming methods for wheat production on this Cache Valley land, it is the general practise to harvest the grain with a header or with a combined harvester and thresher, either of which implements leaves on the land the major portion of the grain straw, which is subsequently plowed under.[2] Mr. Stewart makes incidental reference to this feature of the agricultural practise in the Cache Valley, but he does not make it clear that in this respect that practise is essentially different from what it is in the dry-land wheat regions of the Great Plains and eastward, where it is the custom to harvest the grain with a binder and remove the larger part of the straw. This omission seems particularly unfortunate, in view of the general, and possibly misleading, inferences that may be drawn from Mr. Stewart's otherwise valuable contribution to knowledge. If, as it seems reasonable to believe, the true explanation of the observed humus maintenance lies in the practise of plowing under each year the large amount of wheat straw, it becomes apparent that similar results are not to be expected where a similar practise is not followed.

C. S. SCOFIELD

U. S. DEPARTMENT OF AGRICULTURE,
January 14, 1911

[2] See Bulletin No. 103, Bureau of Plant Industry, U. S. Department of Agriculture, pp. 31–35, issued May 31, 1907.

SPECIAL ARTICLES

SOME EXPERIMENTS ON THE PRODUCTION OF MUTANTS IN DROSOPHILA

MACDOUGALL has reported the successful production of mutations by treating the ovaries of certain plants chemically or osmotically. As long as the full account of his results is not available, it is not easy to judge to what extent it is possible to produce mutations at desire with his method. Tower has apparently succeeded in producing in various species of *Leptinotarsa* certain color mutations at desire by submitting the beetles, during the period of the growth of the eggs, to different degrees of temperature and moisture from those in which they usually live. Gager mentions that by treating the pollen or ovaries of *Œnothera* with radium, some of the new plants were entirely different from the mother plant. Morgan has published the statement that a number of the interesting mutations of *Drosophila,* which he has recently described, came from a culture which had been treated with radium.

The following experiments were undertaken for the purpose of forming a conception concerning the degree of certainty with which mutations can be produced experimentally. We tried the effects of a constant and comparatively high temperature, of radium and of Röntgen rays. The stock of *Drosophila* which we used in these experiments was given us kindly by Dr. Lutz, to whom we wish to express our thanks.

1. *Effects of High Temperature.*—Several culture dishes with *Drosophila* were put into a thermostat, the temperature of which remained constant within 1° around 30.5° C. We found that at higher temperatures we lost a large number of cultures. In the fifth generation of flies, kept in the thermostat, on February 16, a number of dark flies appeared. They were mated with normal ones of the same culture. Some of these cultures were kept in the thermostat and others were brought into room temperature, to see whether at a lower temperature they would continue to breed true. This has now been the case for five

generations. Darkness is recessive to the normal yellow and is not sex limited. Our dark mutation is possibly identical with Morgan's "melanotic" mutant.

On the seventh of March we began to repeat this experiment with the necessary control at room temperature. On April 10, we found in the first filial generation of the control culture kept at normal temperature a dark specimen. None of eleven new cultures kept in the thermostat have thus far given rise to a dark or any other type of mutant. Since then dark individuals were found in another control culture.

From these experiments we must draw the conclusion that a constant temperature of 30.5° does not necessarily produce mutations in *Drosophila,* and second, that a dark form of *Drosophila* may arise "spontaneously," that means by forces at present unknown.

2. *Experiments with Radium.*—A very large number of experiments with radium were undertaken, because it happened that the first culture which we treated with radium chanced to give us mutants. We succeeded in producing short-winged specimens in two different cultures by treating them with radium; while thus far we have not yet observed this mutation in cultures not treated with radium. The manner of appearance of this short-winged mutation was in both cases the same. In the second filial generation of the flies treated with radium, one or more short-winged males appeared. The various forms of mating were tried and yielded the result that the short-winged condition is a sex-limited character. The wild normal males were found to be heterozygous in regard to short wingedness. Thus our short-winged mutant behaved like, and is probably identical with the "miniature"-winged mutant discovered by Morgan. We have now bred the short-winged males and females for five generations and find that they remain constant.

We expected that we might succeed in producing short-winged mutants at desire, but in this we failed. Although we treated more than two hundred different cultures with radium we only observed the appearance of the short-winged mutation in the two cultures, although we repeated the conditions of our successful experiments quite frequently. In both successful cases we submitted the animals only for one or two hours to the action of radium. In one of the two cases the newly-hatched imago alone, males and females were treated for two hours with a weak radium preparation (10,000 units) which was coated with collodium. It is possible that the alpha rays may have affected the animals. In the second successful case a strong radium bromide preparation (over 1,000,000 units) in a glass tube was applied for one hour to a mixture of imago, eggs and young larvæ.

In five different cultures of flies treated with radium the dark mutation appeared, but, while the short-winged mutants appeared in both cases in the second filial generation, there was no regularity in regard to the appearance of the dark mutants.

In one culture treated with radium a white-eyed female appeared in the first filial generation; it is possible that the existence of a white-eyed male in a previous generation may have escaped our notice. In two radium cultures we observed the pink-eyed mutants, but this was also found in cultures not treated with radium.

3. *Experiments with Röntgen Rays* have given us thus far no mutants.

Our results can be summarized as follows:

1. A large number of cultures of *Drosophila* were treated with high and constant temperature, with radium, and with Röntgen rays. Four types of mutations were observed; a dark form (which was the most common), a pink-eyed, white-eyed and short-winged form.

2. In the control cultures, which had not been treated, the dark and the pink-eyed mutations were also observed. As far as the white-eyed mutation is concerned, it is probable that it originated before the treatment of the culture with radium.

3. The short-winged mutants have appeared thus far only in the cultures treated with radium, namely in two cultures out of several hundred. We did not succeed in producing the short-winged mutation at desire by treat-

ing the cultures with radium.

We wish to express our thanks to Mr. Berlinicke, of the firm of Hugo Lieber & Co., who was kind enough to loan us the radium used in these experiments, and to Mr. Bagg, who assisted us in our observations.

JACQUES LOEB
F. W. BANCROFT
ROCKEFELLER INSTITUTE FOR
MEDICAL RESEARCH

AN EXPERIMENT IN DOUBLE MATING

IN my "Inheritance in Silkworms, I.," (1908)[1] I called attention (pp. 37–39) to the beginnings of an experiment in double mating. Only the F_1 generations following a few matings had been obtained at that time, but they gave such promise of interest that I determined to continue the experiment and to widen it. I have now in hand the notes on 85 silkworm broods belonging to this double mating experiment series of 1910. Some of these broods are the F_4 generation from the original 1907 double matings, while others are F_2 or F_1 generations from the original 1908 or 1909 double matings. Taken together the notes of these various 1907–10 rearings from double matings are sufficient to pose some suggestive queries.

By the double mating of silkworms I mean the mating of a female of one race with two males representing different races, one of them usually of the same race of the female, the other of another race. Races are chosen which are readily distinguished by a difference in cocoon color, as yellow or white, or in larval pattern, as banded and unbanded. The silkworm is polygamous and polyandrous, both males and females usually mating more than once before egg-laying begins. Or this repeated mating may continue after egg-laying has begun.

Moths to be experimentally double-mated are reared from carefully isolated cocoons, and the two matings are made to take place immediately following one another for equal or definitely determined unequal periods of coupling, and always before any egg-laying by the female. The young produced from the eggs of each double-mated female are reared isolated in separate trays, which are covered over during the later larval life (possible straggling time).

In any consideration of the results of such repeated mating the unusual way in which the eggs of insects (at least of the silkworm moth and hosts of others) are fertilized must be remembered. This way is, simply, that the male fertilizing cells, the spermatozoa, are received by the female at mating into a special sac or receptacle, the spermatheca (there may be several spermathecæ, as in flies) in which the spermatozoa remain alive and active. This spermatheca, a diverticulum of the oviduct, is situated near its external opening, the vagina. As the unfertilized eggs of the moth pass slowly down from the ovarial tubes into the oviduct they lack only fertilization to be entirely ready for development. They have already their full supply of yolk, they are already enclosed in their protecting envelopes (vitelline membrane and outer, firmer chorion). But these envelopes do not completely enclose the egg-mass; there is, at one pole of the egg, one or more small openings, the micropyle, through which the spermatozoa, issuing from the duct of the spermatheca as the eggs pass, enter the eggs. As soon as a single spermatozoan has entered, a jelly-like substance closes the micropyle and prevents polyfertilization.

Thus when the silkworm moth first mates she receives in her spermatheca, and holds there, a considerable number of spermatozoa representing the heritable characters of the male involved. When she couples again she receives another lot of spermatozoa, and if the second coupling is with a male of different race from the first these spermatozoa represent a new set of characters. What is going to be the result of this double mating as exhibited in the offspring?

It seems, at first thought, that this result

[1] "Inheritance in Silkworms, I.," *Leland Stanford Junior University Publications*, University Series, No. 1, 89 pp., 4 plates, 2 text figs., 1908. Address Librarian, Stanford University, California.

should be nothing new; nothing surprising. We know already what to expect in any simple mating of different races of silkworms. As regards larval pattern and cocoon color the inheritance behavior is usually Mendelian. "Moricaud" (all dark) larval pattern is dominant over "tiger," or banded, pattern; banded pattern is dominant over unbanded (all light). Yellow cocoon color is dominant over white. And the relation of dominant and recessive is of the usual Mendelian character in F_1, F_2, F_3 and succeeding generations. Now although there are two kinds (two races representing these alternative larval and pupal characters) of spermatozoa in the spermatheca of double-mated females, presumably but one spermatozoid finds its way into the egg, and fuses its nuclear matter with the egg nucleus. That is, the female, although double-mated, is presumably only single-fertilized.

As a matter of fact the inheritance behavior of the F_1 and succeeding generations derived from these double-mated females does not seem to bear out the simple presumption just stated. The presence in the body (spermatheca) of the female of two kinds of spermatozoa seems to disturb matters. The comfortable simplicity and regularity of Mendelian inheritance fails to maintain itself. The troubles of irregularity which have not been wholly wanting even in single mating silkworm experiments—and which I have termed in my account of several years' experience of these matings, "strain and individual idiosyncrasies," a term not looked on with favor by thorough-going Mendelians—these irregularities are accented in the double-mating experiments. The irregularities indeed almost assume a seeming of regularity; a non-Mendelian regularity, if such a heresy is admissible.

I shall not try to give here the full data of my 85 "double-mating" lots of 1910. But I shall give a considerable number of examples and a general statement of these results. Later, if worth while, all the data can be given; together, I may add, with the results of three or four years' more work in single-mating crossings to test further the inheritance of certain egg, larval and cocoon characters, already pretty fairly determined by the original series of several years whose results have been published.

I shall limit the examples referred to in this paper to matings among three races and shall refer only to cocoon characters. The three races are Istrian Yellow, a strong Austrian race producing large golden yellow cocoons; French Yellow, a race producing smaller, salmon yellow cocoons; and Bagdad White, a Turkish race producing large pure white cocoons. Bagdad White is a race whose white cocoon color, instead of being regularly recessive to yellow in crossings with yellow cocooning races, is sometimes dominant, as I have shown in my 1908 report (pp. 24–25, section on "strain and individual idiosyncrasies"). All these races have been bred as pure races by me for the last ten years; that is, races faithfully transmitting certain larval and cocoon characters.

In 1908 a Bagdad White female was mated with a French Yellow male from 9:55 A.M. to 11:55 A.M. and with a Bagdad White male from 11:55 A.M. to 1:55 P.M. The young, reared in 1908, were all white cocooners. One mating (1908) (single) among these young produced (1909) 111 white cocoons and 44 yellow cocoons; another produced all white cocoons; also another produced all white cocoons. A mating (1909) between two white cocooners out of the 111 white and 44 yellow lot, produced (1910) all white cocoons. A mating of two yellow cocooners produced (1910) 8 white cocoons and 40 yellow cocoons; another produced 9 white cocoons and 29 yellow cocoons. A mating (1908) between two white cocooners of one of the all white cocoon lots produced (1909) all white cocoons; and so did another. A mating (1909) of two white cocooners of one of these all white cocoon lots produced (1910) 15 white cocoons and 2 yellow cocoons (sick lot); another produced all white cocoons. A mating (1909) of two white cocooners from the other all white cocoon lot produced (1910) 17 white cocoons and 4 yellow cocoons; and another produced 14 white cocoons (both small sick lots).

In 1907 a Bagdad White female was mated with a Bagdad White male from 9:30 A.M. to 10:55 A.M. and then with a French Yellow male from 10:55 A.M. to 12:15 P.M. From this mating there were produced (1908) 25 white cocoons and 13 yellow cocoons. Mating (1908) two of these white cocooners together produced (1909) a small lot divided equally between white cocoons and yellow cocoons (sick lot). Mating (1909) two of these white cocooners together produced (1910) a small lot of white cocooners containing one yellow cocoon (straggler?). (Wherever my records show a single yellow in an otherwise white lot or a single white in an otherwise yellow lot I prefer "straggling" to any other explanation!) A mating (1909) of two yellow cocooners produced (1910) a small yellow lot containing one white cocoon. A mating (1908) of a white and a yellow from the 25 white, 13 yellow lot produced (1909) a small lot composed equally of white and of yellow cocooners. Mating (1909) two of these white cocooners together produced (1910) 23 white cocoons and 2 yellow cocoons.

In 1907 another Bagdad White female was mated with a Bagdad White male from 9:30 A.M. to 10:55 A.M. and then with a French Yellow male from 10:55 A.M. to 12:15 P.M. From this mating there were produced (1908) 33 white cocoons and 19 yellow cocoons. Mating (1908) two of the yellows produced (1909) a small lot equally divided between yellow and white cocoons. Mating (1909) two of these white cocooners produced (1910) 9 white cocoons and one yellow. Mating (1908) a yellow and a white from the half yellow, half white F_1 generation produced (1909) 22 white cocoons. And mating (1909) two of these together produced (1910) a small lot of all white cocooners. Mating (1908) another yellow with another white from the F_1 generation produced (1909) 6 white cocoons and 10 yellow cocoons. Mating (1909) two of these yellow cocooners produced (1910) a small lot equally divided between yellow and white cocoons. Mating (1908) two white from the F_1 generation produced (1909) a small lot all white except for a single yellow cocoon. And mating (1909) two of these whites together produced (1910) an all white lot of cocoons.

In 1907 a French yellow female was mated with a French yellow male from 9:15 A.M. to 10:45 P.M. and then with a Bagdad white male from 10:45 A.M. to 12:15 P.M. From this mating there were produced 57 white cocoons and 74 yellow cocoons. Mating (1908) two yellows of this F_1 generation produced (1909) 22 yellow and 8 white cocoons. Another similar mating (1908) produced (1909) 23 yellow and 2 white cocoons. And still another produced 14 yellow and 5 white cocoons. Mating (1908) a yellow and a white of the F_1 generations produced (1909) 17 yellow and 19 white cocoons. Another similar mating (1908) produced (1909) 21 yellow and 17 white cocoons. Mating (1909) two whites of the F_2 generation produced by two yellow parents produced (1910) an all white lot. Mating (1909) two more whites of this F_2 lot produced (1910) another all white F_3 lot. Mating (1909) two yellows from this same F_2 lot produced (1910) an all yellow lot. Mating (1909) another pair of these F_2 yellows produced (1910) 20 yellow and 9 white cocoons. Mating (1909) two whites of the F_2 generation produced by a white $\times$ yellow produced (1910) 25 whites, 11 yellows and a double cocoon spun together by a white cocooning larva and a yellow cocooning larva. Another mating (1909) of two whites from this same F_2 lot produced (1910) 19 white cocoons and 6 yellow cocoons.

In 1907 a French Yellow female was mated with a Bagdad White male from 9:40 A.M to 11:10 A.M. and then with a French Yellow male from 11:10 A.M. on to the death of the moths. This mating produced (1908) 14 white cocoons and 140 yellow cocoons. Mating (1908) two of the white cocooners produced (1909) 30 white cocoons and 10 yellow cocoons. Mating (1909) two of these F_2 generation white cocooners produced (1910) 62 white cocoons and no yellows. Mating (1908) two more of the F_1 generation white cocooners produced (1909) 20 white cocoons and 10 yellow cocoons. Mating (1909) two of the F_2

generation white cocooners produced (1910) a small sick lot, partly white and partly yellow. Mating (1909) two of the F_2 generation yellow cocooners produced (1910) a small weak lot of 12 yellow cocoons and 2 white cocoons. Mating (1908) two of the F_1 generation yellow cocooners produced (1909) 18 yellow cocoons and 2 white cocoons. Mating (1909) two of the F_2 yellow cocooners produced 4 yellow cocoons and one white cocoon. Mating (1908) two more of the F_1 generation yellow cocooners produced (1909) 59 yellow cocoons and no white cocoons. Mating (1909) two of these F_2 yellow cocooners produced (1910) 47 yellow cocoons and no white cocoons. Mating (1908) a yellow cocooner and a white cocooner of the same F_1 generation lot produced (1909) 18 white cocoons and 14 yellow cocoons. Mating (1909) two of these F_2 white cocooners together produced (1910) 29 white cocoons and 11 yellow cocoons. Mating (1909) two of the yellow cocooners of the F_2 lot produced (1910) a sick lot of 3 white cocoons and 1 yellow cocoon. Mating (1908) another yellow and white pair of the same F_1 lot produced (1909) 10 white cocoons and 10 yellow cocoons. Mating (1909) two of these white cocooners produced (1910) 93 white cocoons and 25 yellow cocoons. Mating (1909) two of the F_2 yellow cocooners produced (1910) 8 white cocoons and 35 yellow cocoons.

In 1907 a French Yellow female was mated with a Bagdad White male from 9:40 A.M. to 11:10 A.M. and then with a French Yellow male from 11:10 A.M. to 12.25 P.M. This mating produced 56 salmon (*i. e.*, pinkish yellow) cocoons and 34 salmon to golden yellow cocoons. (All of these in the general category yellow but varying in shade from pinkish yellow to deep old gold yellow). Mating (1908) two salmon cocooners produced (1910) 13 salmon cocoons and 3 white cocoons. Mating (1909) two of these F_2 salmon cocooners produced (1910) all salmon lot. Mating (1908) another salmon F_1 pair produced (1909) 17 salmon cocoons and 8 white cocoons. Mating (1909) two of these F_2 salmon cocooners produced (1910) 10 salmon cocoons and 6 white cocoons.

So much for double matings between Bagdad Whites and French Yellows. Now for a series between Bagdad Whites and Istrian Yellows.

In 1907 an Istrian Yellow female was mated from 9 A.M. to 10:30 A.M. with an Istrian Yellow male and then from 10:30 until 12 with a Bagdad White male. This mating produced (1908) 55 yellow cocoons and one (straggler?) white cocoon. Mating (1908) two of the yellow cocooners produced (1909) 10 white cocoons and 23 yellow cocoons. Mating (1909) two of these yellow cocooners produced (1910) 2 white cocoons and 12 yellow cocoons. Mating (1908) another pair of yellow cocooners of the same F_1 lot produced (1909) 13 white cocoons and 28 yellow cocoons. Mating (1908) still another yellow pair from the same F_1 lot produced (1909) 10 white cocoons and 24 yellow cocoons. Mating (1909) two of these white cocooners produced (1910) 4 white cocoons and no yellows. Mating (1909) two yellow cocooners of the same F_2 lot produced 16 white cocoons and no yellow cocoons.

In 1908 an Istrian Yellow female was mated with a Bagdad White male from 10:30 A.M. to 12 M. and then with an Istrian Yellow male from 12 to 1:30 P.M. This mating produced 30 yellow cocoons. Mating (1909) two of these yellows produced (1910) 16 yellow cocoons and 4 white cocoons. Mating (1909) another pair of the F_1 yellows produced 19 yellow cocoons and 4 white cocoons.

In 1907 a Bagdad White female was mated with a Bagdad White male from 9:45 A.M. to 11 A.M. and then with an Istrian Yellow male from 11 A.M. to 12:15 P.M. This mating produced (1908) 15 white cocoons and 57 yellow cocoons. Mating (1908) two of these white cocooners together produced (1909) 11 white cocoons and no yellows. Two other pairs of white cocooners from the same F_1 lot produced (1909) small all white lots. From each of these three all white F_2 lots was mated (1909) one pair, and each mating produced (1910) a very small weak all white lot. Mating (1908) two yellow cocooners from the original F_1 generation lot produced (1909) 22 yellow cocoons and 6 white cocoons. Mating

(1909) two of these white cocooners produced (1910) 35 white cocoons and no yellows. Mating (1909) two yellow cocooners produced 24 yellow cocoons and no whites. Mating (1908) another pair of F_1 yellow cocooners produced (1909) 9 white cocoons and 20 yellow cocoons. Mating (1909) two of these white cocooners together produced 25 white cocoons and no yellows. Mating (1909) two of the yellows together produced (1910) 12 white cocoons and 21 yellow cocoons. Mating (1908) another pair of yellow cocooners from the original F_1 lot produced (1909) 4 white cocoons and 17 yellow cocoons. Mating (1909) two of these yellow cocooners together produced 8 white cocoons and 20 yellow cocoons. Mating (1908) a yellow cocooner and white cocooner of the original F_1 lot produced (1909) 12 white cocoons and 12 yellow cocoons. Mating (1909) two of these white cocooners produced 19 white cocoons and no yellows. Mating (1909) two of the yellow cocooners produced (1910) 10 white cocoons and 17 yellow cocoons. Mating (1908) another yellow and white pair produced (1909) 16 white cocoons and 20 yellow cocoons. Mating (1909) two of these white cocooners produced (1910) 6 white cocoons and no yellows. Mating (1909) two yellows produced (1910) 8 white cocoons and 6 yellow cocoons. Mating (1909) a yellow and a white produced (1910) 4 white cocoons and 12 yellow cocoons. Mating (1908) another yellow and white pair produced (1909) 5 white cocoons and 31 yellow cocoons. Mating (1909) two of these whites produced 19 white cocoons and no yellows. Mating (1909) two of the yellows produced 9 yellow cocoons and no white cocoons.

In 1907 a Bagdad White female was mated with an Istrian Yellow male from 9:45 A.M to 11 A.M. and then with a Bagdad White male from 11 A.M to 12:15 P.M. This mating produced 41 white cocoons, many of them creamy white instead of the pure or faintly greenish-white characteristic of the Bagdad white race. Mating (1908) two of these white cocooners produced (1909) 59 white cocoons. And mating (1909) two of these F_2 white cocooners produced (1910) a small all white lot. Similar F_2 and F_3 all white lots were obtained from another F_1 mating. Mating (1908) another pair of F_1 white cocooners produced (1909) 46 white cocoons and 15 yellow cocoons. Mating (1909) two of these white cocooners produced (1910) a small all white lot. Mating (1909) two of these F_2 yellow cocooners produced (1910) an all yellow lot.

In 1907 a Bagdad White female was mated with an Istrian Yellow male from 9:45 A.M. to 11 A.M. and then with a Bagdad White male from 11 A.M. to 12:15 P.M. (This was an exact duplicate of the 1907 double mating just described.) This mating produced (1908) 48 white cocoons and 20 yellow cocoons. Mating (1908) two of these white cocooners produced (1909) a small all white lot, and a mating (1909) of two from this lot produced (1910) a smaller all white lot. Mating (1908) another white pair from the F_1 generation produced an all white lot, and a mating (1909) of two from this lot produced (1910) a small all white F_3 lot. Mating (1908) two yellow cocooners of the F_1 lot produced 9 white cocoons and 12 yellow cocoons. Mating (1909) two of these F_2 white cocooners produced a small all white lot, while mating (1909) two of the yellow cocooners produced (1910) a very small all yellow lot. Another mating (1908) of two yellow cocooners of the original F_1 lot produced 26 yellow cocoons and one white cocoon and mating (1909) two of these F_2 yellow cocooners produced (1910) 14 yellow cocoons and 2 white cocoons. Another mating (1908) of two yellow cocooners from the original F_1 lot produced 28 yellow cocoons and 12 white cocoons. Mating (1909) two of these F_2 yellow cocooners produced 50 yellow cocoons and no white ones, while mating (1909) two of the white cocooners produced (1910) 15 white cocoons and 1 yellow cocoon (straggler?). Mating (1908) a yellow and a white from the original F_1 lot produced (1910) 40 white cocoons and 16 yellow cocoons. Mating (1909) two of these white cocooners produced (1910) 28 white cocoons and 29 yellow cocoons, while mating (1909) two of the F_2 yellow cocooners produced (1910) 5 white cocoons

and 34 yellow cocoons. Another mating (1908) of a yellow cocooner and a white cocooner from the original F_1 lot produced (1909) 20 white cocoons and 19 yellow cocoons. Mating (1909) two of these white cocooners produced (1910) 70 white cocoons and no yellow ones, while mating (1909) two of these F_2 yellow cocoons produced (1910) 6 white cocoons and 9 yellow cocoons.

In 1907 a Bagdad White female was mated with a male Istrian yellow from 9:40 A.M. to 10:45 A.M. and then with a male Bagdad White till death of the moths. This mating produced (1908) 29 yellow cocoons. Mating (1908) two of these yellow cocooners produced (1909) 25 yellow cocoons and 8 white cocoons. Mating (1909) two of these F_2 whites produced (1910) a small all white lot. Mating (1908) another pair of the F_1 yellow cocooners produced (1909) 6 white cocoons and 12 yellow cocoons. Mating (1909) two of these F_2 white cocooners produced (1910) an all white lot. Mating (1909) two of the yellow cocoons produced 9 yellow cocoons and 6 white cocoons. Mating (1908) another pair of the F_1 yellow cocooners produced (1910) 30 yellow cocoons and 9 white cocoons. Mating (1909) two of these yellow cocooners produced (1910) an all yellow lot. Mating (1908) still another pair of the F_1 yellow cocooners produced (1909) 19 yellow cocoons and 4 white cocoons. Mating (1909) two of these F_2 yellow cocooners produced (1910) 29 yellow cocoons and 5 white cocoons.

These are the records. Their interpretation may be made by any one interested. In scrutinizing them for significance this should be remembered. In ordinary (single) matings of Bagdad White with Bagdad White only white cocoons are produced in F_1 and all following generations. In mating French Yellow with French Yellow or Istrian Yellow with Istrian Yellow only yellow cocoons are produced in F_1 and all following generations. In mating Bagdad White with Istrian Yellow usually all the cocoons of the F_1 generation are yellow. Mating these together usually produces in F_2 generations 3 yellow to 1 white, the Mendelian behavior. In mating Bagdad White with French Yellow the dominance of yellow is not so steadfast. There is, as I have shown and particularly emphasized in my 1908 paper, more or less aberration from the Mendelian rules in this mating. And indeed, these aberrations are likely to occur in any other crossing of silkworm races. The usual inheritance behavior of silkworm cocoon characters is, however, Mendelian. The aberrations constitute what I have called "strain and individual idiosyncrasies." This simply means that I believe that there is more in the order of inheritance than is covered by Mendelism. The Mendelian elements in this order are becoming recognizable and familiar. The other elements are not yet so obvious to us.

In these double matings the aberrations are abundant and conspicuous. After a double mating the whites of the F_1 generation mated with other whites of the same generation do not always produce whites. They may produce both yellows and whites. Or this latent carrying of the yellow character by these presumably strictly recessive (white) carriers may not be manifest till an F_3 generation. What does this mean?

In seeking an answer, the state of affairs as regards actual fertilization in these double mating cases must be kept in mind.

The female receives during an hour's or two hours' coupling a large number of fertilizing cells from a male of her own race (and hence her own cocoon characters). She then receives during another hour's or two hours' coupling a presumably equal number of germ cells from another male of different race (and different cocoon characters). These two lots of active spermatozoa are held in the spermatheca. Is one group above or in front of the other, so that when an egg arrives opposite the opening of the spermatheca it will necessarily be fertilized by one of this upper or front group (the group provided by the second male)? Or do the actively motile spermatozoa become thoroughly mingled in their fluid vehicle so that access to the egg will be according to the law of probabilities? More likely the latter alternative should prevail.

When, however, a spermatozoan enters the egg through the micropyle this micropyle should, by analogy with the observed conditions in various other insect eggs, become closed, thus preventing poly-fertilization. If this is so then a double mating should after all result in but a single fertilization, and these fertilizations should be roughly divided between the two male types.

Thus in double mating a female Bagdad White with Bagdad White and Istrian Yellow males, the fertilizations should be, roughly, equally divided between pure race Bagdad White and crossed Bagdad and Istrian Yellow. And in accordance with these fertilizations half of the F_1 generations thus produced should be white cocooning and half yellow cocooning (yellow being dominant in crossings with white). If an Istrian Yellow female is mated with both Istrian Yellow and Bagdad White males F_1 generations should always be composed of all yellow cocooning individuals. Or if in these double matings all of the fertilizations are effected by spermatozoa of one of the males only then the F_1 lots should be either all white cocooning or all yellow cocooning. F_2 generations from these lots should follow the Mendelian order and break when the F_1 individuals are hybrids and not break when they are pure race progeny.

But the data given above do not reveal the expected behavior. They evidence a considerable perturbation in the order of inheritance. The F_1 lots are not always all white or all yellow, or equally divided between white and yellow as they seemingly should be. Or if such all white or all yellow F_1 lots are produced, they often throw both yellows and whites in F_2 lots when only yellows or only whites should have appeared. Or if they do produce all white or all yellow F_2 lots intermating in these lots may produce both yellows and whites in F_3 lots. In a word the inheritance behavior is not that which it should be in animals usually following a Mendelian order, if the only influence at work on the egg is the nuclear content of a single pure race spermatozoan.

What, then, is causing this pertubation in the order of inheritance? Do the eggs in double-mated females receive more than one spermatozoan and are these spermatozoa often the representatives of both the races used in the double mating? Or can the egg be in any way influenced by the mere presence in the spermatheca of spermatozoa representing both of a pair of allelomorphic heritable characters? Can fluids carrying the spermatozoa have any influence during fertilization? Can the spermatozoa of one type influence those of the other type during their enforced companionship for several hours or days in the female spermatheca?

All that we think we know of the mechanism of fertilization and heredity makes us answer "No" to each of these questions. Then why should the order of inheritance in the silkworm moth be different in the generations after these double matings from the order in the generations following a single mating?

Vernon L. Kellogg
Stanford University, Cal.

SOCIETIES AND ACADEMIES

THE NATIONAL ACADEMY OF SCIENCES

At the stated meeting of the academy on April 18–20, the following papers were read:

"On the Motions of the Brighter Helium Stars," W. W. Campbell.

"Report of Progress in Spectrographic Determinations of Stellar Motions," W. W. Campbell.

"The Evolution of Periodic Solutions of the Problem of Three Bodies," F. R. Moulton.

"Mechanical Quadratures," G. F. Becker.

"Corollaries of the Theory of Isostasy," W. M. Davis.

"Experimental Investigation on Reflection of Light at Certain Metal-liquid Surfaces," Lynde P. Wheeler (introduced by C. H. Hastings).

"On the Origin of the Peaks of Maximum Pressure in the Midst of the Permanent Tropical Oceanic Highs," W. J. Humphreys (introduced by Cleveland Abbe).

"A Further Study of Columbic and Tantallic Oxides," E. F. Smith.

"The Outlook of Petrology," J. P. Iddings.

"The Orogenic Development of the Northern Sierra Nevada," Waldemar Lindgren.

"Biological Conclusions drawn from the Evolution of the Titanotheres," H. F. Osborn.

"A New Reptile from the Newark Beds," W. B. Scott.

"Restorations and Ontogeny of the Eurypterids," J. M. Clarke.

"A Geological Reconnaissance in the Rocky Mountains of British Columbia," Chas. D. Walcott.

"Comparative Study of the Early Stages of Vertebrates," C. S. Minot.

"Infantile Paralysis and its Mode of Transmission," Simon Flexner (read by C. S. Minot).

"Cell-size and Nuclear-size," E. G. Conklin.

"The Cause of Death of the Unfertilized Egg and the Cause of the Life-saving Action of Fertilization," Jacques Loeb.

"Studies of the Pulmonary Circulation," Horatio Wood, Jr. (introduced by H. C. Wood).

"An American Lepidostrobus," J. M. Coulter.

"Aristotle's History of Animals," Theo. Gill.

"Notes on New England Mollusca," E. S. Morse.

"Changes in Bodily Form of Descendants of Immigrants," Franz Boas.

"Classification of Shoshonean Tribes," C. Hart Merriam.

"The Outside and the Inside of the Yosemite Indian," C. Hart Merriam.

"Biographical Memoir of W. H. C. Bartlett," E. S. Holden.

"Biographical Memoir of C. B. Comstock," H. L. Abbot.

"Biographical Memoir of S. W. Johnson," T. B. Osborne.

"Biographical Memoir of Benjamin Silliman, 1816–1885," A. W. Wright.

"Biographical Memoir of James H. Trumbull," A. W. Wright.

"Biographical Memoir of C. A. White," Wm. H. Dall.

"Biographical Memoir of Joseph Leidy," Henry Fairfield Osborn.

THE AMERICAN MATHEMATICAL SOCIETY

THE one hundred and fifty-third regular meeting of the society was held at the University of Chicago on Friday and Saturday, April 28–29, 1911, the occasion being especially marked by the presidential address of Professor Maxime Bôcher on "Charles Sturm's Published and Unpublished Work on Differential and Algebraic Equations." This was the first regular meeting of the society, except the summer meetings, that has been held elsewhere than in New York city. The attendance exceeded all previous records, reaching a total of 115, including 88 members. Fifty-three papers were presented at the four sessions.

The president of the society, Dean H. B. Fine, of Princeton University, occupied the chair, being relieved by Professor G. A. Miller and Vice-president G. A. Bliss. The council announced the election of the following persons to membership in the society: Professor H. Bateman, Bryn Mawr College; Mr. Samuel Beatty, University of Toronto; Professor J. H. Griffith, University of Michigan; Mr. E. J. Moulton, Harvard University; Mr. George Spitzer, Agricultural Experiment Station, Purdue University; Professor C. J. West, Ohio State University. Eleven applications for membership in the society were received.

Professor L. E. Dickson was reelected to the editorial board of the *Transactions* for a term of three years. A committee was appointed to arrange for the summer meeting and colloquium to be held at the University of Wisconsin in 1913.

Friday evening was devoted to the usual dinner, at which 73 members were present.

The following papers were read at this meeting:

Daniel Buchanan: "A class of periodic solutions of the problem of three bodies, two of equal mass, the third moving in a straight line."

H. E. Buchanan: "An expansion of elliptic functions with applications."

D. R. Curtiss: "Relations between the Gramian, the Wronskian, and a third determinant connected with the problem of linear dependence."

L. L. Dines: "On the representation of resultants of polynomials in one variable."

L. L. Dines: "On the solution of three equations for three variables in terms of others."

W. D. MacMillan: "A reduction of a system of power series to an equivalent system of polynomials."

W. D. MacMillan: "A method for finding the solutions of a set of analytic functions in the neighborhood of a branch point."

R. L. Moore: "On the transformation of double integrals."

Maxime Bôcher (presidential address): "Charles Sturm's published and unpublished work on differential and algebraic equations."

L. P. Eisenhart: "A fundamental parametric representation of space curves."

A. E. Young: "On certain orthogonal systems of lines and the problem of determining surfaces referred to them."

Arnold Emch: "The differential equation of curves of normal stresses in a plane field."

A. B. Frizell: "A set of postulates for well-ordered types."

C. J. Keyser: "Sensuous representation of paths that lead from the inside to the outside of

an ordinary sphere in point four-space without penetrating the surface of the sphere."

Edward Kasner: "The subdivision of curvilinear angles."

R. D. Carmichael: "The general theory of linear q-difference equations."

R. D. Carmichael: "Note on multiply perfect numbers."

G. A. Miller: "Isomorphisms of a group whose order is a power of a prime."

R. G. D. Richardson: "Theorems of oscillation for two self-adjoint linear differential equations of the second order with two parameters (second paper)."

J. B. Shaw: "Quaternion functions of three parameters."

J. E. Rowe: "The combinants of two binary cubics and their geometrical interpretation on the rational cubic curve."

U. G. Mitchell: "Geometry and collineation groups of the finite projective plane $PG(2, 2^2)$."

G. E. Wahlin: "The decomposition of rational primes into ideal prime factors in the field $k(\sqrt[p]{m})$."

L. C. Karpinski: "An Italian Algebra of the fifteenth century."

C. H. Sisam: "On hyperconical connexes in a space of r dimensions."

R. E. Root: "Iterated limits of functions on an abstract range."

E. B. Van Vleck: "On the generalization of a theorem of Poincaré."

E. B. Van Vleck: "On the classification of collineations."

A. R. Schweitzer: "On the philosophy of Grassmann's extensive algebra."

A. R. Schweitzer: "On the 'working hypothesis' in the logic of mathematics."

W. B. Ford: "A set of sufficient conditions that a function may have an asymptotic representation in a given region."

W. J. Montgomery: "The classification of twisted curves of the fifth order."

William Marshall: "On Hill's differential equation in the theory of perturbations."

H. Bateman: "The fundamental equations of the theory of electrons and the infinitesimal transformation of an electromagnetic field into itself."

N. J. Lennes: "Curves and surfaces in analysis situs."

N. J. Lennes: "Extension and application of a theorem of Ascoli."

L. I. Neikirk: "Substitution groups of an infinite degree and their related functions."

James Maclay: "Parabolic curves."

J. A. Nyberg: "Projective differential geometry of rational cubic curves."

E. B. Stouffer: "Invariants of linear differential equations with applications to ruled surfaces in five-dimensional space."

W. D. MacMillan: "A general existence theorem for periodic solutions of differential equations of a certain type."

A. R. Crathorne: "The catenary with variable end points."

F. R. Moulton: "Periodic orbits of superior planets."

F. R. Moulton: "On the curves defined by certain differential equations."

F. H. Safford: "An identical transformation of the elliptic element in the Weierstrass form."

W. H. Roever: "Southerly deviation of falling bodies (third paper)."

C. N. Moore: "Convergence factors in double series."

L. E. Dickson: "On the negative discriminants for which there is a single class of positive primitive binary quadratic forms."

L. E. Dickson: "On Fermat's 'descente infinie.'"

L. E. Dickson: "On perfect numbers and Bernoullian numbers."

O. E. Glenn: "On expressing a quantic in terms of assigned powers of a given quantic."

G. R. Clements: "Implicit functions defined in the neighborhood of a point where the Jacobian determinant is zero."

R. W. Marriott: "Determination of the groups of isomorphisms of the groups of order p^4."

The summer meeting of the society will be held at Vassar College on Tuesday and Wednesday, September 12–13.

F. N. Cole,
Secretary

THE AMERICAN PHILOSOPHICAL SOCIETY

At the stated meeting of the society on April 7, Rear Admiral G. W. Melville (U. S. Navy, retired) read a paper on "A Century of Steam Navigation."

The author said: In looking back over the history of the human race we are struck with the fact that from time to time some genius brings to light, or develops a principle which forever after is a guide in our thinking. Such was Lord Bacon's exposition of inductive logic, which has been the basis of all scientific advancement.

Basing his argument on the above theory, the admiral followed the growth of steam navigation from the time of the inventions of James Watt, down to the time of Fulton's first commercial steamer *Clermont*, down to the time of the *Lusitania* and *Mauretania* of to-day.

He spoke of the varied improvements from that time up to the present time, including the many improvements, not only in ships and ship building from the wooden hulls to the present steel hulls, but the engines and boilers through their various stages of improvements, commencing with a low steam pressure of 10 pounds to the square inch up to the present time of about 300 pounds pressure per square inch.

He traced through the various steps the great evolution of steam ships from those of about 500 tons to those of 40,000 tons. Of necessity in a short lecture of but 40 or 45 minutes, a very

rapid tracing of the growth of steam navigation and marine engineering had to be quite limited. Nevertheless, the lecture seemed to be of great interest to his audience. And the fact of the growth of steam ships from the paddle wheels used to a large extent in the forties of the last century, through the propeller systems, and finally of the great advances made through the steam turbine, which is only an improvement of Heroes's steam turbine of 2,000 years ago.

He mentioned the famous steamer *Great Eastern*, which was a wonder in her day, which is nearly a half century ago. She was simply about fifty years in advance of her time, for although a great engineering success, she was not a commercial success, which is the real measure in these days of what is considered a success in commercial life.

He laid great stress on the improved material with which the engineer could work to-day, without which it would be impossible to build the great vessels of 30,000 tons displacement, and 70,000 horsepower.

Another fact which he pointed out, and which is worth noting, was that high speeds properly belong to big ships, because experiments had shown that for a higher speed the resistance of a large ship per ton of displacement was very much less than that of a small one.

He dwelt upon the different types of battleships, from the *Dreadnought* type down to the small torpedo boat and torpedo-boat destroyer. Stating that in naval construction it was necessary to have the various classes of ships to fill their particular positions in the battle fleet, the same as the different arms of the service in the army, such as artillery, cavalry, infantry, etc.

After the lecture illustrations were made of the lecture proper, by means of a series of illuminated lantern slides.

THE PHILOSOPHICAL SOCIETY OF WASHINGTON

THE 694th meeting was held on April 22, 1911, Vice-president Fischer in the chair. The following paper was read:

The Scientific Aspects of the President's Inquiry into Economy and Efficiency: Dr. F. A. CLEVELAND, chairman, Committee on Economy and Efficiency.

The work that the committee had been asked to do was mentioned, and which, briefly stated, is that it was to make such concrete recommendations to the president as would enable him to act with greater economy and efficiency in the management of the business affairs of the government.

At the beginning there was little of scientific information of how the large business concerns of the government are organized or what the government is doing. The committee felt a grave responsibility. It assumed in the start that for progress and commendable results it was necessary to focus the attention of all in the service upon the subject of administration. This required a working hypothesis or common plan of cooperative effort or coordination. As the whole of this inquiry looks to something constructive, it first had to be decided what sort of information is needed by the man who is responsible for the conduct of the government's business.

The first work was to find out how the government is organized and what it is doing. The president asked each head of department to cooperate with the White House, and under their supervision the inquiries have been conducted. Diagrams and charts were exhibited showing the departmental organizations in their different divisions and branches and their activities, and how they are connected up and coordinated; these being based upon the reports and outlines of organization prepared by the several departments.

In describing the organization of the government's work, the speaker remarked in passing that one is really amazed when he knows what wonderful organizations some of the government offices are.

Reports were also secured describing the legal powers of the various departments and divisions of the government service throughout the country in order to ascertain the authority for the various activities pursued, and in these matters also the committee had appealed for their information to the men in the service who had been living the parts; to those directly concerned.

The committee assumed that the cost of the government's activities should be known, and that all should know that the administration of the government's affairs is economical. To ascertain this it is necessary to know, (1) that a thing is bought, (2) what is bought and (3) is it suitable for the purpose for which it is bought. Also in studying economy the relation of cost to results must be known.

The analysis of the costs of the government's activities in terms of administration, operation and maintenance were discussed, and how the analysis was applied in arriving at standards of judgment of costs in relation to results.

R. L. FARIS,
Secretary